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SUBCOMMITTEE ON EXTREME EXTERNAL PHENOMENA

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UNITED STATES NUCLEAR REGULATORY COMMISSION

ADVISORY COMMITTEE ON REACTOR SAFEGUARDS

SUBCOMMITTEE ON EXTREME EXTERNAL PHENOMENA

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Sheraton Inn International
Conference Center
11810 Sunrise Valley Drive
Reston, Virginia

Thursday, January 28, 1982

The Subcommittee on Extreme External Phenomena convened at 8:30 a.m.

PRESENT FOR THE ACRS:

DAVID OKRENT
J. CARSON MARK
HAROLD ETHERINGTON

DESIGNATED FEDERAL EMPLOYEE:

RICHARD SAVIO

P R O C E E D I N G S

MR. OKRENT: This meeting will now come to order.

This is a meeting of the Advisory Committee of the National Safeguards Subcommittee on Extreme External Phenomenon.

My name is David Okrent, the subcommittee chairman. Other ACRS members present today are Mr. Etherington and Mr. Mark. We also have in attendance several ACRS consultants, Drs. Luco, Maxwell, Page, Wilson, Pomeroy, Thompson, and Trifunac.

The purpose of this meeting is to examine the uncertainties associated with the determination of a design basis earthquake for a nuclear power plant at the site in the Eastern United States and the needs for research in this area. The agenda has been strutured to encourage comments on these issues from the audience. I engourage you to participate.

The meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act and the Government in the Sunshine Act.

Dr. Richard Savio is the Designated Federal Employee for the meeting.

The rules for participation in today's meeting have been announced as part of the notice of this meeting previously published in the Federal Register on Thursday, December 31, 1981.

A transcript of the meeting is being kept and will be made available, as stated in the Federal Register notice. It is requested that each speaker first identify himself or herself and speak with sufficient clarity and volume so that he or she can be readily heard.

I would like to welcome all of you. Several years ago, the ACRS held a subcommittee meeting in Los Angeles to review our knowledge of seismicity in the U. S. east of the Rockies, and this meeting is intended to be a sequel to that one. We would like to learn about the significant developments of the last several years as they relate to our overall knowledge of seismicity in the U. S. east of the Rockies and as they relate to specific regions and locations. We are interested in gaining further insight into the magnitude of shaking as a function of increasing the smaller frequencies, for example, one in 1,000 per year, one in 10,000 per year, one in 100,000 per year, et cetera.

In this regard, the uncertainties in any such estimates may be of equal interest as the estimates themselves.

Finally, as many of you know, the NRC safety research budget has been subjected to a significant cut in fiscal year 1982 and may be cut again in fiscal year 1983. A considerable part of the seismic and geological work previously supported by the NRC has been terminated. We are very interested in attaining your considered opinions as to what seismic and geological research should be funded by the NRC, at what cost, and why.

We would like to have specificity in such recommendations, if possible, showing how some important issue or question will be addressed directly by the proposed research.

We have a very ambitious agenda. There is a little bit of leeway to run later than scheduled tonight.

(General laughter.)

MR. OKRENT: And there is not much place to go. And tomorrow it is scheduled to end at 4:00 o'clock. We might possibly run until 4:30. Since questions, comments, and discussions represent a vital part of a meeting such as this, I must ask each speaker to take no more time for his formal presentation than is shown on the agenda. Otherwise, he may not get a chance to give his conclusions unless they come first.

I plan to run the meeting very informally, giving all attendees the opportunity to question or comment, so please raise your hands and try to participate as much as you feel you would like to.

With that, I will get into the agenda. I assume everyone has a copy.

MR. SAVIO: There are copies on the end of the table, if they don't.

MR. OKRENT: All right. There are copies of the agenda at the end of the table, I am told.

The first brief speaker is Mr. Beratan from the NRC.

MR. BERATAN: On behalf of the Office of Nuclear Regulatory Research, I would like to welcome you, and I would like to pay particular tribute to my predessor in this position, Dr. Jerry Harbour, who in my opinion did an excellent job in putting this whole research program together.

The objectives of the seismological and geologic research programs are really threefold. One is to provide an improved data base necessary to reduce the uncertainties about the seismological and geological risks in licensing decisions by making broad use of probabilistic risk assessment, and to provide data necessary for the development of standards on seismic design input applicable to the safety of nuclear facilities, and three, to provide data necessary to revise Appendix A, the tendency of our Part 100, and to develop the appropriate guidance necessary to deal with issues experienced in case review.

To bring you up to date on the status of the research in the earth sciences, we are nearing completion of the first five-year phase of the program in the geologic and tectonic studies of New England, New Madrid, and the Nemaha Ridge. We will hear later on from some of the principal investigators in these areas.

As you know, we are funding regional micro-earthquake networks in New England, in the Charleston area, in the New Madrid area, and the Nemaha Ridge. We have a very important question that faces us. The question is this. How long must we continue to monitor these networks, five years, ten years, six years, whatever, to gather a sufficient data base for the work which we have in mind?

Dr. Pomeroy and Mr. Anderson of Maine Survey will bring us up to date on some of the recent activities in New England and some of the more recent earthquakes.

Some of our current plans, because of our recent budget cuts, coupled with the lack of new applications and conflicting priorities within the Office of Research, some hard decisions had to be made.

We are able in fiscal year '82 to preserve the funding for the operation of the micro operation networks, but we were forced to stop additional work in the area of geology. Hopefully, if some money gets loosened up in fiscal '83, we will be able to go back and pick up some of the geologic work.

In the future, we will continue to review and reorder priorities, and hopefully to enter into some cost sharing agreements with other federal agencies, states, utilities, and industries interested in the type of seismic and geologic data we are gathering.

This is a brief summary of where we are and where we are going. Thank you.

MR. OKRENT: All right. Let's go right on, then, to the next speaker, Mr. Jackson from the NRC also.

MR. JACKSON: My name is Bob Jackson. I am chief of the Geoscience Branch in the Office of Nuclear Reactor Regulation, and we deal primarily with the licensing of nuclear power plants, and we provide to the Office of Research, Mr. Beratan's group, a sort of user need request, and I was thinking this morning about how to summarize user needs. I think it can be done rather simply.

Basically, what we need is a good way of predicting earthquakes, when they will occur, what size they will be, and what the ground motion from them will look like after they do occur. So other than that, we have a very simple request. There are a number of items we put together. I think these try to incorporate in them ongoing research which is already under way, and I will just run down through them.

I have handouts available of this vu-graph, which indicates the background going with each of these. Some will be self-evident, and I won't spend any time on them. Others I will briefly go through.

I have three general groups. One is ongoing research related to tectonic province design basis approach, and the association of earthquakes with structural or tectonic provinces as required in Appendix A, 10 CFR, Part 100, which is our siting regulation. The second group is research related to regional tectonics and faults. The third broad group is related to research related to ground motion determination.

Essentially, Point A, the seismic zoning map of siting nuclear facilities, Drs. Pomeroy, Nutley, Barstow, and Brill worked on another attempt at developing a "tectonic province map" for the eastern United States for the NRC staff, with some degree of success. I think Dr. Pomeroy will speak about this later. We would like to see a continuation of a program such as this. It is self-evident.

Again, the Charleston, South Carolina, earthquake area. This earthquake area, based on a staff position, has been kept at the Charleston area for purposes of siting and licensing plants in the eastern United States, and our understanding is that a large earthquake could affect a large number of plants in the eastern United States.

New Madrid earthquake area. The studies there have been very productive to date and have helped us take a good look at the relationships of earthquakes to structure and the USGS and state programs have assisted us in allowing us to deal with that New Madrid seismic zone.

A great deal of work and effort has gone into the New England seismo-tectonic program, supervised essentially by Pat Barosh. He will speak about that also.

From a staff point of view, we feel the integration of this into some sort of decision-making role is extremely important, and we would like to see an increased focus on those areas of specific seismicity. We were thinking about the moodus noise events of a year or two, but of course a week or so ago we had the northern Maine, New Brunswick area earthquake, which may be even more productive for the area.

Item E, Nemaha structure earthquake area, this is a broad structure which runs through the region which has some association with earthquakes in that broad region structure, but we continue to need to understand that more. and the Ohio earthquake area, monitored there for a long period of time. There were very few earthquakes. In the last year or two, we have begun to get a few earthquakes. It still appears we don't have a good idea of the relationship of these earthquakes to structure in the region.

In this last, this general group is the Mt. St. Helens volcanic area. As you might know, we have the Trojan plant located approximately 35 kilometers from Mt. St. Helens, to the west of it. So we have an ongoing problem there in monitoring Mt. St. Helens and being aware of its impact not only on that facility but others in the Pacific Northwest.

Of great interest over the last many years has been, the Ramapo Fault, Indian Points 2 and 3 reactors, and the association of seismicity with this complex geological structure that runs through that region. There are a number of workers working in this area, and we feel there can be very productive work and research done on this, to indicate again an association of the better understanding of earthquakes with structure, broad regional structure in this case in that area.

I was speaking to someone this morning and telling them that Item B is always of interest to us, because back not too many years ago all geology for a nuclear facility stopped at the shoreline, and everything else was blue out there. There was no geology in the ocean. I think that has been reversed, and we are trying to accumulate now, and we feel there could be a comprehensive compilation of off-shore geology near all facilities or proposed facilities.

In the northwestern United States, seismo-tectonics is very difficult. It combines a bit of California type geology with eastern type geology, especially in terms of making siting decisions in that area. The geology there is at the early stages of understanding. We feel a great deal of effort needs to be put into that area.

Utilities in that region are working hard on this aspect. The NRC has begun to do a little bit of work in that region, but does not do very much.

The next item is one which we have again not paid much attention to except in specific licensing cases. This is growth faulting in the Gulf Coast region. There are questions about whether these are seismic features in terms of low-level seismicity. Determining their rate of movement, potential for renewed activity, relationship to ground water extraction and the like has become a problem for some plants, and further work is needed.

The fault parameter compilation relates to sites in the western United States in which relationships like Slemmons' 1977 Corps of Engineers paper which compiled looking at faults and trying to get from the geology to some sort of predictive capability in the seismology, to deal with taking known parameters and relating to predicting maximum possible earthquakes, regional possible earthquakes, and the like.

This has been a consideration in many ACRS meetings and proceedings we have had.

MR. OKRENT: Bob, if you use two more minutes, you will have used ten minutes.

MR. JACKSON: All right. I am sorry.

Finally, this last group, and it is pretty self-evident, it relates to ground motion, and again, I will just run through quickly. We need development of site-specific, standardized response spectra. We need further consideration into seismicity and seismic design. We need increased work on attenuation relationships in the central and eastern United States.

We need source characteristics of central and eastern United States earthquakes.

In the last several years, a great deal of work has been done on modeling of earthquakes in the near field or near source, and we feel this should be continued. One near Dr. Okrent's heart is the treatment of uncertainty in ground motion estimates, and we feel some continued work in probability estimates needs to be done.

A new area is shallow earthquakes in the eastern U. S. These are shallow, high damage earthquakes which have occurred, such as in 1965, the southern Illinois earthquake.

And finally, since we are now utilizing a great deal of magnitude relationships and we have an extensive data base of intensity, we need to begin to relate intensity to magnitude.

Thank you.

MR. OKRENT: Thank you.

Let's go right on, then, to the next speaker. Mr. Hamilton of the USGS will discuss the geological origin of eastern U. S. seismicity.

MR. HAMILTON: The last time I gave a talk about the geologic origin of eastern seismicity, I was speaking in Knoxville, Tennessee, to an audience made up of engineers and managers, with a few geologists and geophysicists in attendance. Therefore, I felt fairly comfortable in attempting to present an overview of eastern seismicity. However, today the situation is quite different, with at least a few people in the audience who know more than I do about every single topic I will cover.

Moreover, many of these people are on the program later, so they will have an oppor unity to publicly point out my mistakes. Nevertheless, I will push on, but exercising some caution, I will focus mainly on the areas I have been working in in recent years, which are the New Madrid area and Charleston, and only touch somewhat lightly on the other areas of the east.

Perhaps with a warning, this morning, when he came in, John Berent gave me a fortune from his Chinese fortune cookie last night which said, "The fool speaks; the wise man listens."

(General laughter.)

MR. HAMILTON: So, with that, I will commence speaking.

First slide.

Well, as noted earlier today, we are concerned with earthquakes in the eastern United States, and just to start off, here is a little comparison from Doug Rankin's paper to show that although earthquakes are not as frequent in the United States, they do cover a larger area. We can compare the areas of intensity, seven and six for the New Madrid earthquakes, with a band of comparable size, the San Francisco 1906 earthquake, and also that of the Charleston earthquake with one of the same size, San Fernando in 1971.

So you can see that although we don't have events as often in the east when they do occur, they affect a much larger area. People working on eastern seismicity have long been frustrated by the scatter of seismicity. This is a figure from York and Oliver, and it shows if you simply take a compilation of earthquakes, an historic compilation, you could see that they are very broadly distributed. This is very discouraging to those who like to find lineations in a pattern, and be able to associate that with known geologic features.

(Pause.)

MR. HAMILTON: The two worst things I imagine about giving a talk are, first, spilling my coffee in my lap right before I get up. The second worst thing is having the projector bulb burn out.

(General laughter.)

MR. HAMILTON: While they are fooling around, I will briefly summarize some of the conclusions I will try to make during this talk. One conclusion is, with regard to the New Madrid area, I believe we have at this point successfully identified the geologic structure responsible for the New Madrid seismicity, namely, a rift that formed probably before Late Cambrian time, possibly in the Protazoic time, although there must be some uncertainty as to whether it could have formed in the Late Paleozoid.

In any case, the New Madrid seismicity is largely occurring within the rift, and at this point I think we understand the class of the seismicity, particularly in relation to the regional stress field. In regard to the Charleston area, in recent times, we have carried out extensive seismic reflection surveys at Charleston both on-shore and off-shore, and although all of that data is not available, I think some of the conclusions are fairly clear.

One conclusion is that the pre-Late Crutaceous unconformity in the Charleston area both on and off-shore is remarkably smooth. This is a 100 million year old surface, and although that surface dips gently from on-shore to off-shore, local relief on that surface is nowhere greater than 50 meters. This means that a

100 million year old surface in the Charleston area has experienced during that time less than 50 meters of depth formation locally. Although we don't fully understand this, I think it is a significant observation. How can you have so little deformation in an area which is supposedly one of the most important seismic areas in the eastern U.S.?

Is that slide projector working now? Or have you given up?

VOICE: The spare bulb is also burnt out. He has gone to the main desk.

MR. HAMILTON: All right.

Those are two of the points I will make. A third point I will make as we go along is, I believe the new results from Giles County which Gil Bollinger will be speaking about are very important. Gil has found that earthquakes in the Giles County area are located at depths ranging from five kilometers to 25 kilometers in depth, which places them well below the Paleozoic cover.

Moreover, he has found a trend in these centers to find a zone that strikes roughly northeasterly, a similar trend as is observed in the central Appalachians, but contrasting with the trend of the southern Appalachians under which the zone lies.

This suggests that the cause of the Giles

County earthquakes associated with perhaps a much older rifting event, perhaps rifting associated with the Proto Atlantic Ocean. I think this is very important in that it presents another possible case of reactivation of very ancient structures.

Toward the end of my talk, I will make an argument that further progress in understanding eastern seismicity, particularly in relation to geology, lies in trying to interpret the tectonics of the east in somewhat of a block tectonics model, similar to what has been used in California and along the San Andreas Fault.

I believe through integration of new gravity maps, magnetic maps, and we can define these blocks with some uncertainty, of course, that we can analyze the configuration of these blocks and their orientation and the orientation of the boundaries between these blocks in terms of the regional stress field, and that such an analysis will yield a prediction of the type of motion one should expect along these blocks, and then by comparing this predicted block tectonic interaction and comparing it with the seismicity, one can perhaps come up with some conslusion about more of a mechanical type model for the east.

Now, I am not so naive as to believe that such an approach will be easy, and I in fact cannot explain

Charleston's seismicity in such a model at the present time, despite all of the work that has been done there. However, I believe that New Madrid seismicity can be explained in such a model, and perhaps some of the other seismicity in the mid-continent area.

The general approach I would argue for is one of integration of regional geophysical data, seismicity data, and stress data to try to come up with some type of a block tectonic model for the eastern United States. I believe this would put eastern, the design of nuclear reactors in the east on a much sounder basis.

This is a plot of the larger earthquakes of the east and a tectonic map of the U. S. This is information I extracted from catalogues by Bollinger, Nuttli, and others. You can see what I have included are earthquakes of an intensity of seven and larger and a fault area of 450,000 kilometers square or larger. These are earthquakes east of the Rocky Mountain front.

The main points are that the zone of most intense activity, the New Madrid area, lies here, at the northern end of the Mississippi embayment. The Mississippi embayment is a major re-entrant ino the North American Crayton, and it is filled with light crustaeous and sinezoic sediments. This is the only area in the eastern United States where earthquakes as large as Intensity 12 have occurred.

The other area of importance in the southeastern U. S. is the Charleston earthquake, which lies under the Atlantic coastal plane. Perhaps it indicates the diversity of nature that the two most important areas are areas covered with a kilometer of mud, which makes it very difficult to find out about the tectonics of these areas.

The Giles County area, another I will mention, you can see here, at the western margin of the Appalachians. Again we are presented with a difficult problem, because it appears the seismo-tectonic feature in that area may be covered with several thousand feet of paleozoic rocks.

The other area, of course, is in eastern Canada, La Maille Bay area, and activity along the St. Lawrence Valley, and activity within the northeastern U. S., in New England. Focusing in on the central, the mid-continent area, this is Otto Nuttli's map, which shows the compilation of earthquake activity in the mid-continent, and again you can see that the main concentration of activity is in the New Madrid area, near the confluence of the Ohio and Mississippi River.

This map shows a fairly broad scattering of seismicity which again makes it difficult to try to associate these features with geology.

Here is a compilation of earthquakes of magnitude four and a half or larger, again from Nuttli's data, and you can see some trends begin to appear that are better defined. Again, the New Madrid concentration, the concentration in southern Illinois and Kentucky, but several other trends are evident, one running down through Kansas and Nebraska, associated perhaps with Nemaha, and a very curious linear trend I will mention briefly later which runs northwest, through this area, other activities in the Ouachtas, and then up in Ohio.

Focusing now on the New Madrid area, these are the larger earthquakes which have occurred in this area. The biggest ones are right under the embayment at the northern end, where the three occurred in 1811 and 1812. This was followed by an earthquake at the southern end in 1843, and another large earthquake near Charleston, Missouri, in 1895, so these together seem to define a linear zone which extends under the northern embayment. Other activity is in southern Illinois.

Tectonically, this is an extremely critical area, because it lies between the Nashville dome and the Ozark uplift, both positive structural features of long duration and negative features, the Illinois basin and the Mississippi embayment. This is also extremely interesting because it lies between the Ouachta origin and the Appalachian origin, and I will speak in a moment about some of the problems associated with the continuity between those origins.

Just north of the Mississippi embayment is the area of most intense faulting in the eastern United States. This includes the faults which sometimes have been grouped as the 38th Parallel lineament, which includes Cottage Grove, the Shawnee Town fault down through there, and the St. Genevieve.

Also, there is a dense network of faulting at the northern end of the embayment, in the Illinois Kentucky mineral district. However, knowledge of how these faults extend under the northern embayment and their character there has been lacking until recent years when seismic reflection profiling has been carried out in that area.

The northern part of the embayment or the whole embayment is an area of positive gravity anomalies, the only area, such a broad area in the east other than along the Atlantic Margin. A seismic reflection profile that was run along the northwestern margin of the embayment by McCamy and Meyer.

Keep the line of this profile in mind, and later on there is a cross-section I will show along this line. The model derived from McCamy and Meyer indicated the presence in the lower crest of a layer having a seismic velocity of 7.4 kilometers per second, indicating a possible intrusion of material into the lower crest. This can be contrasted with a profile by Stewart in northern Missouri which shows a more typical crustal profile.

Irvin and McGinnies tied together the gravity data and the seismic reflection profile to define a crest that looks something like this. Just pay attention to this part of it (indicating), and the key feature of it is that they concluded there was this layer, Number 6, which is this layer of the 7.4 kilometer per second velocity in the lower crest, and from this they concluded this was a rift type structure.

They anchored their line in the west with the McCamy and Meyer line, and the rest is based on gravity data. Notice that in the middle, the pillow of material in the lower crest comes up to about 25 kilometers.

A little over a year ago, we ran extensive refraction profiles in the embayment. These are the lines we ran, but I simply want to point out that the axial line, this line right along here, the preliminary results from that seem to confirm that the 7.4 kilometer per second layer exists at about 23 kilometers depth. This is being worked on by Bill Ludder, a USGS employee who is now at Purdue.

So, the preliminary results would tend to confirm the type of model that Irvin and McGinnis came up with. The magnetic data from the -- second vertical derivative map, Tom Hilinbrand's data, and just for orientation, you can barely see the boot hill in there. The Mississippi River comes through here (indicating). The most impressive feature is this band of s dued relief which has been interpreted to indicate the presence of a gravin. This is a down grout part of the crystalline crust which has a relief of about two kilometers.

So, to kind of summarize the New Madrid results, we have defined a rift as indicated by BB prime and AA prime. This rift is bounded by furred plutons and the New Madrid seismicity lies down the axis of the rift with an intense zone that offsets and runs up into Kentucky. I believe Mark Zoback or someone later will refer to the nature of the stress field in this area.

I just want to briefly show some of the important data we have gotten. We run a seismic line down here in Arkansas, and on this seismic data you can see the sediments down to the Paleozoic surface at that level, which is about one kilometer deep. And then we have evidence where the nature of the Paleozoic structure down under the embayment. This was the first data publicly available, or it still is the only data, I believe, publicly available showing structure down in the Paleozoic rocks, and we can define some beds that dip up.

Now, this is dipping up to where the seismic zone (indicating). These would be Orovician, Cambrian, and perhaps pre-Cambrian rocks, and as you come into the seismic rocks you get a band of incoherent data, and on the other side of the seismic zone, you can see some reflections with cross-reflections. These are unmigrated, so these probably should belong up here (indicating). As you move further east, you move into some flat line beds down into the Paleozoic.

This deformation is indicative of a period of deformation which came some time after the Ordovician and before the Late Cretaceous. I have been interested lately in the continuity between the southern Appalachian and the Ouachta. This is a model from Bill Thomas from the University of Alabama, who proposes that when the Proto-Atlantic opened this area was essentially pulled apart and transformed along the southern boundary, and rifts in this area.

This is an idea that Phil King had also dealt with. He shows a transformed fault at the southern end of the Appalachians extending up into the Ouachta area. Why are these features important? They are important because it says something about the southern end of the New Madrid seismic zone. If the New Madrid seismic zone was formed as a rift in the late Pre-Cambrian at the same time as the Proto-Atlantic Ocean formed, or during the same tectonic event, this implies that that rift is terminated at the southern margin of the North American Crayton.

Similarly, during the end of the Paleozoic, during continental conversions, there should be a suture zone along the southern margin, so that would argue the New Madrid seismic zone could be terminated at the suture zone.

This is a new magnetic map from Izzy Zekes. I am showing it because you can see a fairly well defined magnetic anomaly running along the southern part of the Ouachtas, and although it is not quite as well defined, it seems to turn and head on down to the southern Appalachians, and I think a plausible interpretation of this would be that this anomaly, this curved lineation marks the southern margin of the North American Crayton, and therefore one might argue that the southern limit of the New Madrid seismic zone occurs right about in here (indicating).

Another interesting feature of this map is, you can see this magnetic low that turns through northeastern Arkansas. This is the magnetic low associated with the New Madrid seismic zone, but you can also see that as you move southeastward, you have a high, a low, a high, and then a low, another high, and then a very subtle low.

So, one question is, could these be other possible rift structures along that area? And should they be considered as potential siting zones?

While I have this on, let's take a look at Charleston, which lies down here. You can see a very sharp linear feature extending from the southern end of the Appalachians. It is a very straight line that cuts right across the northern end of the Charleston area, and then the southern end of the Charleston area is defined by the so-called Brunswick magnetic anomaly, which swings in in connection with the east coast magnetic high.

Also notice that the train, the type of magnetic train that exists in the Charleston area also extends up along the Atlantic margin.

Well, let's see. I will quickly point out a few. I wanted to note these earthquakes that occur in these trends and if you pick out just these earthquakes plus the one down here, these occur on something called the Colorado Lineament, as defined by Warner. This again is a feature that has Pre-Cambrian origins. Maybe this is a coincidence, but it is true that in Nuttli's catalogue, all of the earthquakes of magnitude 4 and a half up in this area (indicating) fall in this feature. Another falls down here on a controversial feature called the Texas Lineament. Plane solutions are available for only two earthquakes. This would indicate left lateral movement here and right lateral movement here (indicating).

I will switch down here to Charleston now quickly. In the Charleston area, this is in North Carolina, but in that area, we have the geologic setting such that we have Cenezoic and Late Crutaceous deposits. The wedge of sediments on the passive continental margin that overlies a crest, a continental block of somewhat undetermined properties. We know that in many areas there are Mesazoic basins underlying this area, because in some places they are exposed and in other cases drill holes have shown this.

These are basins which were formed at the time, in the Mesazoic, the opening of the Atlantic, and we believe that in the Charleston area such a basin underlies that region. This shows the configuration. This is from Ken Clipboard. This shows the configuration of the continents at the time of opening. You can see a number of these basins indicated in green along the margin in the Charleston area.

This is another magnetic map. The Charleston area is right about in here. There are strong magnetic anomalies. Based upon magnetic data, this was thought to be a Triassic basin in this area. We now have the seismic line across South Carolina, and we can see evidence on the seismic line of a basin as you come off this magnetic anomaly.

So, our preliminary interpretation is that we would infer that underlying the Charleston area there is a thick section of Triassic red bed deposits. I believe this will be addressed later.

The seismic coverage we have in the Charleston area is indicated by these black lines, both on-shore and off-shore, except that since the slide was prepared we now have about twice as much coverage. We have much more coverage off-shore and about double the coverage in this area. John Brenner will discuss this later, but I will just make one quick point.

Off-shore we have found a fault which has subsequently been confirmed which we call the Helen

Banks fault. We can trace this for 40 or 50 kilometers, and it has a displacement of 30 or 40 meters. It is about the only main fault we have defined in this off-shore area. We have defined no north-west trending structures in this area.

In the Charleston area, up around Summerville, we have found several very minor faults which seem to appear in a zone flexture that extends in a northeasterly direction, pretty much through the Summerville area. These are not very impressive faults, but they do seem to appear primarily in this zone, where the pre-Crutaceous surface or a Durrasic age of basalt, however you want to look at it.

I mentioned earlier in my conclusions that a major finding here is the lack of deformation of this 100 million year old surface which must be explained. This is a slide from Lynn Sykes, and I just raise the point that one possible explanation for some of the seismicity has been related to the fracture zone, and I just want to note we did not find structures we could relate to the extension of the fracture zone toward the continent.

This is a slide that summarizes some of the problems of explaining the Charleston earthquakes. Here we show the wedge of sediments that overlie the area.

Underneath, we know that we have a basalt flow of Durrasic age, 116 million years old, I guess, and also in this area we know we have red bed deposits of Triassic and Durrasic age. We can see minor faulting that upset the fault, and one major question is, what do those faults do at depth? Do they become listric, flatten out onto a surface, or do they extend on down?

It should be pointed out that most of the seismicity is between 3 and 13 kilometers, so it is down in this area, where we have little information of the geologic structure at this time. Several proposals have been made about the existence of decoma or detachment surface extending under the region. Segram Armbruster has discussed this. The idea has been discussed by Cook, Harris, and others further to the east.

On our marine data, we do see some iefractions, and short reflections that suggest there may be some kind of surface down around 10 kilometers and the possibility, I think, must be considered that there is some type of a detachment surface under the Charleston region, although I don't feel the evidence at the present time is very conclusive on this issue.

Let's see. Maybe I should stop at that point.

MR. OKRENT: You are legally allowed at least three minutes, and we must give you a few more minutes because you lost your slide projector, so why don't you assume you've got five minutes?

MR. HAMILTON: I will take it.

Turning away from Charleston now -- well, let me go back to Charleston for one moment. I do feel I should point out that most of the mechanisms discussed as a cause for the Charleston earthquake are ones which are pervasive along the eastern margin. For example, if ultimately Charleston seismicity is attributed to reactivation of Mesozoic basins, we have many Mesozoic basins along the Atlantic margin. If we end up concluding that Charleston seismicity is associated with a decoma or a detachment, such a detachment was presumably formed at the time of the Paleozoic closing of the Atlantic, and such surfaces should exist in many places along the Atlantic margin.

Similarly, if the location of Mesozoic basins is somehow controlled by earlier sheer zones or earlier structures, these are also major features that exist along the Atlantic margin. The uniqueness of Charleston, I think, hinges largely on the past occurrence of an intensity 10 earthquake there, and the existence of the Charleston block, but the magnetic pattern shows that a similar magnetic trend extends elsewhere along the Atlantic margin.

I will skip over Giles County.

Since I referred to it, I will quickly mention the trend identified by Gil Bollinger is a trend such as this, and you can see this trend is similar to the central Appalachian rather than the southern Appalachian, and I will let Gil deal with the rest of that. I will skip the northeastern U. S. I want to point out some data accumulating primarily from Paleomagnetic results by Vandervo and others that are beginning to show that large-scale left lateral movement perhaps occurred along the Atlantic margin.

The magnetic evidence indicates that some time after the Debonium, perhaps as much as 1,500 kilometers of left lateral movement may have occurred along some sheer zones on the Atlantic margin. The sheer zones are perhaps covered now by later tectonic events, but they are features that may become important in identifying seismicity.

Now, to begin to wrap this up, this is a figure Don Anderson published in Scientific American over ten years ago, but it is one which has always intrigued me. This is his model of block tectonics for southern California, and I think as time has gone on, this model has proven more and more correct, at least in my mind it has, where you can view the interaction along the margin as the interaction of a number of small plates rather than any type of single surface, and that the tectonics of this region really is the tectonics of interaction among these various blocks.

The reason I raise this model is, I wonder if a similar type approach could be made in the eastern United States. The main difference is that in the west, you have a well-understood tectonic engine which produces steady motion over tens of millions of years. There is a steady progression of plate motion associated with the interaction between the North American and Pacific plates, whereas in the east we are dealing with motions that are much less.

For example, the mid-continent gravity high, a feature formed perhaps late Pre-Cambrian. Although there are offsets in the mid-continent gravity high, they do not amount to more than 100 kilometers. Therefore, the types of deformation permissible in the mid-continent area are much less than along the Atlantic margin. Nevertheless, I wonder if such an approach might be useful in the mid-continent.

Here is the new magnetic map of Izzy Zekes, and although I realize you cannot see the details on here, there are many features that can be correlated very closely with geology in this map. Also, the work being done at Purdue, I think, Bill Hines and his group, points out the correlation between many major features and the magnetic data. This offers the possibility of delineating some of these crustal blocks. Also, new compilations of gravity data are becoming available.

This is the Bouger map of Hanna and Barnes. But the most interesting gravity data, I think, are the derivative maps being produced. This is a filtered map that Marty Kane and his colleagues have produced, where the short wave length anomalies have been passed, and what this tends to bring out is upper crustal structure. You can see a very sharp delineation of mid-continent gravity high. You can see a good delineation of the southern Oklahoma lockagen with trends along the edge of the Crayton here and many other features.

These maps are going to be set up on posters here, and people can take a look at them later, but this type of data, I think, offers the opportunity to delineate these blocks and add additional information to the analysis of seismic risk in the eastern United States.

Thank you.

MR. OKRENT: Thank you.

(Applause.)

MR. OKRENT: I have had a question posed to me. Can we get copies of some of these illustrations? What is the chance?

MR. HAMILTON: Which ones?

VOICE: All.

VOICE: Any part.

MR. HAMILTON: I could have copies made of the slides, if that is what you would like.

MR. MAXWELL: Is there any way of getting a Xerox copy of the slides?

MR. OKRENT: They wouldn't be in color.

MR. MAXWELL: Yes. Perhaps they wouldn't be legible. I am sorry. I thought perhaps you had some avilable. If not --

MR. HAMILTON: Many of these, as you can see, were slides of figures published by other people, but it would be easy to run off copies of the slides if that would be convenient.

MR. OKRENT: All right. We have some time for discussion of this first paper.

Yes. Please go to a microphone, and give your name, and fire away.

MR. HERMAN: I am Bob Herman from St. Louis University.

MR. OKRENT: I am told there is a switch there.

MR. HERMAN: I also believe it doesn't work. I will just sr up. I would like to know, Bob, what do you think future directions for research should be? You have given us a very broad overview. Do you have any idea what problem areas might be solved in the near future or where we should go in the future?

MR. HAMILTON: Well, I think the way to approach future research is to look at the past and see what has proved to be successful. I think the New Madrid area is a success story. I think that a very critical element in New Madrid was your seismograph network. I think that the improved resolution in the seismicity provided the key to unraveling the New Madrid problem. It showed instead of just a shotgun pattern of seismicity, you in fact had lineations in the pattern, and this removed some of the mysticism, I think, about New Madrid seismicity.

So, I would argue that I think the seismograph networks are very important. The problem in most of the east is the low rate of occurrence of seismicity. So, there is a question of whether the government is willing to make the long-distance run, or whether they will pull out the networks every time the budget turns down.

So, I think a policy decision on the part of the government must be made with regard to this approach. There is no point in setting up a network unless you stick with it, because it takes a long time to build up the data base, and the other approach I think is very important is the other items I mentioned, the improvement of regional geophysical data and the integration of it with geologic data, and I think the stress program is very important as well.

MR. CHINNERY: Chinnery, MIT.

Bob, you talked about the structure and the earthquakes. I am interested in the connection between these. You mentioned reactivation of ancient structures. I am interested to know what you mean by that, and I guess also, is there any evidence yet that any of these structures have been reactivated in some way?

MR. HAMILTON: I think the best case for reactivation is the New Madrid area. We have there a structure that we believe was originally formed in Late Pre-Cambrian or probably Pre-Late Cambrian, and we know that that structure was -- there was tectonic activity in that area that we can see on seismic data after the Ordovician and before the Late Crutaceous. We know there is permean volcanic activity, ignous, intrinsic activity at the north end of that zone. We know the most active episode of faulting at the northern end of the embayment occurred toward the end of the Paleozoic, in Pennsylvanian predominantly, about the the time of the strong activity in the Ouachta. We know at magnet -- that there was Late Crutaceous, ignous activity which occurred within the embayment, and we have evidence from seismic data there was faulting which occurred perhaps as late as the same time in the New Madrid area.

These are all tectonic events I am referring to, and you might say, does this necessarily mean seismic activity? I think that argument has to hinge on the close association of current seismicity with the rift structure that was identified. So I think a very convincing case can be made for reactivation in that area. Elsewhere, I think it is more tenuous, but I think an excellent review of this has been made by Lynn Sykes, and I think overall it is a concept which I find very convincing.

MR. RYDER: Leon Ryder, NRC. Bob, I will give you a hypothetical question. Let us set up a scale from zero to ten. The thing we are looking at is the correlation of earthquakes with geologic structures. For ten, let's talk about, let's set the standard as the San Andreas Fault, and for zero at this point, before we hear from New England, let's assume we know nothing about the earthquakes that recently occurred in New

Brunswick. Where would you put New Madrid and Charleston each in that numerical range, and which structure would you associate it with.

MR. HAMILTON: I would put New Madrid on the upper end of your scale, you know, eight or nine. I think we have a very good understanding of the relationship of New Madrid activity with geologic structure. What we don't understand of New Madrid as well is the long-term tectonic deformation affecting the area. The San Andreas Fault we think we understand very well, because of plate motion. In New Madrid, we are talking about interplate earthquakes. The rates of deformation are much lower, and although we think we know something about the stress field primarily from fault plane solutions, we know in that area the motion on many faults goes up for a while, goes down for a while. The motion changes.

So, I think what we lack at New Madrid is the larger tectonic setting. At Charleston, I think we are very much on the lower end of your scale, because although we have identified certain types of geologic structure that exists in that area, and although we have certain hypotheses that might possibly explain the activity, at this time I don't think a very certain explanation has been advanced, although I think a number of the hypotheses are certainly worth considering.

So, I would put Charleston at 2 or 3, something like that.

MR. TRIFUNAC: Dr. Trifunac, University of Southern California.

I hope you can correct me. I thought you said the most models, geologic models that could be used to explain the Charleston setting are very common in the United States, and the only unusual feature is the MMI-10 earthquake there.

MR. HAMILTON: Yes. I said the uniqueness of Charleston has usually been based on the fact that that is the only area along the Atlantic coastal plain where an intensity 10 earthquake has occurred. That is one argument for the uniqueness of Charleston.

The other argument for the uniqueness of Charleston that I have heard is associated somehow with the Charleston block. As you can see on the magnetic map, there seems to be a somewhat distinct magnetic signature in the Charleston area.

What I am raising is to me that does not in itself provide a tectonic explanation for that earthquake. It strikes me that the fact it is unique is because an intensity 10 earthquake occurred there may not be circular reasoning, but I don't see the logic behind that; and you certainly wouldn't apply that kind of logic in California on uniqueness. You would look more in the tectonic setting of the area.

But the other argument about the uniqueness based upon the Charleston block, I think as we have acquired more magnetic data and as a lot of the marine data have become available, you can see a similar type of magnetic terrain exists along much of the eastern seaboard.

Furthermore, we know now that much of the magnetic signature that you see in the Charleston area is in fact reflective of Mezosoic tectonics, and we know the Mezosoic tectonics of that same type occurred in many areas along the Atlantic margin.

So those arguments of uniqueness with me don't carry much weight. What I am looking for is an explanation for the earthquake in terms of a tectonic model, and the hypotheses I am aware of are those I summarized, and those are ones that are processes that were associated with openings and closings of oceans, and as such were processes that affected large areas. So that's the way I feel about it.

I presume you raised that question because you disagree with my point of view. Is that right?

MR. TRIFUNAC: No. To the contrary. I just wanted to be sure that that's what I understood.

MR. PAGE: Bob, may I carry that a little further?

Couldn't you say the same about New Madrid? If there hadn't been those two very strong or that series of very strong earthquakes there, would there have been any reason for you to go in there and be shooting now?

MR. HAMILTON: If it were seismically quiet --

MR. PAGE: Well, there are many other areas in the U.S. which have small quakes at the present time.

MR. HAMILTON: Yes. Well, do you want to answer that one for me?

MR. HERRMANN: I think excluding the New Madrid earthquakes of 1811 and 1812, if you look at the historic seismicity pattern for the last 112 years that there's more there than anywhere else.

MR. PAGE: More than in the northern St. Lawrence Valley, for example?

MR. HERRMANN: He had a slide of Otto Nuttli's catalog for the earthquakes in the central U.S., and just looking at that slide, New Madrid would have only been three points on there, and you would notice there is a dense concentration there.

Whether we would be worrying about New Madrid if those earthquakes had occurred 50 years earlier than that the way we do today is another question, because we would be ignorant of the fact that a large earthquake had occurred.

MR. PAGE: That is the point I am making with respect to the Charleston earthquake. It seems to me the two are roughly comparable.

MR. HERRMAN: Yes. If Charleston had not occurred --

MR. PAGE: Len is shaking his head.

MR. HERRMAN: It is an interesting question whether we are ignoring regions for the same fact that something has not occurred for the last 100 or 200 years. But on the other hand, present day seismic activity tells us that it's more active than anywhere else and perhaps it has a potential.

MR. OKRENT: Along the lines of the last comment I just ran across a note that said there is a recent paper by Kirkland and Rogers entitled "Earthquake Potential in Colorado," in which the authors report that quakes of up to magnitude 7 and 1/2 have occurred repeatedly during recent geologic time.

Will someone talk about that on this agenda?

(No response.)

MR. OKRENT: Well, would someone think of it?

(Laughter.)

If you would like to have a look at this note which appeared in the Natural Hazards Observer put out by Gil White at the University of Colorado, I would be happy to let someone look at it.

MR. HERRMAN: Dave, Rost may be able to address it or Algermisson.

MR. OKRENT: Algermisson will not be here; he is ill. So if someone else would take the trouble, I would appreciate it.

I guess we should probably move along. We can come back to Mr. Hamilton perhaps, if you wish, in the next discussion period or the succeeding one. We don't have to keep our discussion periods rigidly confined to the topic immediately preceding the open discussion.

In any event, I guess we had best proceed, and we are going to move into the New England-Northeastern U.S. area. Mr. Barosh is scheduled to be the next speaker.

MR. BAROSH: It will take a minute to load the slides.

MR. OKRENT: Well, while he is loading the slides are there any other additional questions or comments for Mr. Hamilton?

Yes, please.

MR. HINES: Bill Hines, Purdue.

I think one of the good examples of what John Maxwell was discussing is the situation with the New

Madrid rift which has the high seismic activity, and then the Rough Creek grabens in the Moorman Syncline which is at right angles right adjacent to the New Madrid.

This is an area where there is very little seismicity. Otto and others have pointed it out. We have much the same type of structure from what we are starting to see, and I think this is a major problem: should we be looking at the Moorman Syncline.

MR. HAMILTON: The way I would explain the lack of activity in the Moorman Syncline is those structures are subparallel with the regional stress field. Therefore, if Zoback and Zoback and other compilations are right, there would not be a sheer component along those features at the present time, whereas there is long the northeast striking New Madrid zone.

Now, one might ask whether the St. Genevieve, which should have a component of sheer that would cause left lateral strikes, may be potentially but it has not in historical time experienced a strong earthquake, although there is some activity in that area.

MR. PAGE: Bob, there is a great variation in trend along that Rough Creek, Kentucky fault, so somewhere in there you would certainly have a component of sheer in any regional stress field.

MR. HAMILTON: That's true, but by and large it is an east-west feature. And I realize there are segments in there that show quite a bit of variation. There is low level activity in certain parts. Certainly if you look on Otto's map there is some activity scattered around. But in further support of this model I am arguing for, in the Wabash Valley area, actually a little west of the Wabash area, there have been two earthquakes Bob has determined fault plain solutions for, and one of his solutions shows a reverse type of solution, a compressional solution, and the other is a predominantly strike-slip solution which indicates lateral motion, and this is on a north-northeast feature. And I think these kinds of motions are consistent with this type of a mechanical model.

At the first reports from the Sharpsburg earthquake in Kentucky, which again is in the vicinity of the northeast trending Lexington fault zone where there is a right lateral solution, that fit with my model, but later on I understand they have changed the fault plain solution. Is it still a right lateral?

MR. HERRMANN: It hasn't changed. Ours has always been the same -- right lateral, northeast.

MR. HAMILTON: All right. So the larger

earthquakes for which we have fault plain solutions and the general activity I think fits reasonably well with this type of a model.

MR. POMEROY: Bob, one more question. Could you go a little further with the implications of what you are saying? How many, for instance, unidentified features might there be in the eastern central United States? It seems to me I remember in Knoxville we were talking about blocks on the order of a few hundred kilometers in linear dimension.

If that kind of thing is true are we talking about large numbers of features in the eastern central U.S. that are oriented favorably in the present-day stress field for reactivation?

MR. HAMILTON: As everyone knows, the continental crust of the eastern U.S. has been ravaged by tectonic processes, so if you look at the magnetic maps, you can see many lineations probably indicative of different stages of rifting and other types of deformation. And if you look at the gravity map, which should reflect more the gross crustal properties, you can see strong lineations.

This shows that there are many, many blocks. It may be important that the largest earthquake in the east occurred on a very major feature. It occurred on a rift that extends for a few hundred kilometers. It is a very major feature. So maybe the first attention could be given to these major features and how they lie in the stress field.

I don't think it is a simple problem at all. I think you will end up with a lot of different features and a lot of potential source zones.

And the other problem is we know from historic reference, for example, in China that certain faults or fault zones are active for periods of time and then they are quiescent for periods of time; so systems turn on and turn off. That is a complicating factor.

All I am really trying to argue here is moving somewhat away from total reliance on historic seismicity and trying to understand the seismic source zones in terms of the tectonic processes occurring, and trying to extend what we know about those tectonic processes to other areas where similar tectonic features exist, and trying then to make a judgment about potential seismicity.

And I don't pretend that that is a simple process. I am just arguing it is a more logical process than just assuming the future will be like the past in terms of seismicity.

MR. OKRENT: I guess we had better go on for now.

Mr. Borash.

MR. BAROSH: I will start with the first slides. We will be talking about this area in the Northeast, a New England seismotectonic study which extends on down to northern Delaware and westward to cover all of New York. I will be attempting to do a number of things today. I hope I don't go too fast and sound like a tobacco auctioneer.

What we have been trying to do in the area over the last six years and where we started, and something of what we think we see today and where we would like to go, and a couple of comments on the recent earthquakes, if I can squeeze that in and probably go overtime a little bit.

One of the things here that you can see is that this is a seismic map there of Hadley and Devine showing frequency, number of events for unit area; and you see quite clearly there two bands. There is a band going down from Anna towards La Mailbaz and on up to activity on the lower St. Lawrence, the band on the coast, and a little connection in the middle there in the Hudson River area where they tend to touch.

You can also see in the upper righthand corner that area with NB on it which shows a little more activity. The upper part of the B was just about where the recent earthquake occurred up there.

Looking at the activity in that area a little more closely it breaks down into a number of clusters. This is an older map, I think of threes and above, that are just little lines drawn around some of these clusters. And as we have improved the catalogs over recent years these clusters have tightened up. A lot of events in between have been spurious. The activity we have been recording in the area has been pretty much occurring right in the clusters, and we have a pretty good idea now that for the most part the activity in the region is somewhat stationary. It isn't moving around a great deal, although activity may increase or decrease from time to time in some of these spots.

When the program started six years ago with the matter of trying to understand what is going on in each of these clusters, unlike the Charleston and the New Madrid area we have about a dozen different earthquake areas in New England. This made the work much more difficult in that there were so many different areas to concentrate on. But it also offered a great opportunity in that we could compare and contrast areas.

We are trying to look over all of these and ask what is similar between them, is there anything similar. And the purpose, of course, was to develop -- Bob was mentioning about these blocks. We were trying to divide up New England into various seismotectonic blocks, devise various boundaries, what might you expect in each one.

What we had to start with was that the area in this slide was covered by very good aeromagnetic data. It was the first region in the U.S. covered by such data, and studies had started 12 years ago in the area. We went out to closely match these with the geologic structures, and we found very good tie-ins with the structure; so we had good aeromagnetic coverage in part. We knew a lot about regional structure. We had very good geologic maps in eastern Connecticut and eastern Massachusetts. These were great advantages. And we proceeded from there to do the work.

So it has been a matter of trying to systematically develop in each area the gravity, the additional magnetics, the seismic history, the local arrays where it covered from surface mapping, looking into activities of surface movements; and these have been continuing in each spot, and I will try to give now just a quick summary and a comment on only a couple of the areas we have been working in.

I would like to mention it has been a

cooperative study with the NRC and state surveys and people in universities. A great number of people have been involved, and contributions for budgets come in from various sources. I will be using these general contours from the Hadley and Devine map shown on a number of slides.

One of the things we recognized and felt very important early was that there is a relationship between the seismicity and the altitude of the region. The great number of earthquakes occur in lowland areas. Principally they are concentrated in bays along the coastline and along the St. Lawrence River in the lowlands areas extending up to some of the other river lowland valleys with some high level activity in the Adirondacks, a minor high land activity in spots in the White Mountains, and actually not circled, the New Brunswick area is another slightly high land activity. But earthquakes were very densely concentrated in lowland bays.

And proceeding to studies, particularly in Maine, we were finding that their surface movements going on, that this is from releveling data, and showing that present-day we have the Passama Quoddy Bay area indicated to be subsiding at a rate of 9 millimeters a year relative to Bangor. Other studies along with this, along with agricultural sites, land data, the licensing marine delta salt marsh studies, the tidal gauge data, all corroborate this.

These have been going on continuously elsewhere. We have been able to find out there are areas of active surface movement through the region. It is even visible.

Here is an old dock up near Passama Quoddy Bay which is covered by salt marsh and water at high tide. You can see the subsidance in this region. Local inhabitants have recognized it over their lifetime.

There have been a number of places we can demonstrate this: in the area we have pointed out here, Passama Quoddy Bay in southern Maine, probably in the Connecticut River body, Raritan Bay south of Brooklyn, uplifts in the Adirondacks, and subsidance in the La Mailbaz area -- all active areas.

We have realized New England actually is an extremely faulted area from the magnetics and from our detailed maps; but it was trying to find out through all of this first to locate the faults and then to sort through what may still be alive, as most of the faults were formed in very early Paleozoic.

One of the things that we have come across was that the high angle faults, grabens, extensional features and Mezosoic faulting we were trying to separate out, and the Mezosoic dike systems. There are some very, very long dike systems. There is one shown here in little black circles. There is a dike system that comes out of Long Island and crosses all of southern New England and goes out around Portland, Maine. It shows a very old deep weakness in the crust. It perhaps even crosses Maine, the Mezosoic volcanic chain of the White Mountains and the east-west Monta region hills and other dikes. But besides the Mezosoic extensional features, we realize the older faults that were extensions were probably important also.

Here now in Passama Quoddy Bay, which was the center of all of the subsidance, you have the Oak Bay fault zone, a northwest trending one. And to the accuracy of the locations of activity, the activity is concentrated in the bay and concentrated along this fault. This is an older map with a couple of additional stations in the last few years. There is a great deal more epicenters that can be put on the center, but the center of this bay is also the center of a Devonian grabens, and a Devonian grabens sits in the center of a great downwork of late solarian volcanics, so this had been an area of ancient subsidance.

From the work of Olley, Gates and others you can see that there are unusual northwest trending faults in this area, particularly the Oak Bay; and these things really stand out on the Landsat and gravity work that has been done in the area.

Oak Bay unfortunately is almost covered by water everywhere, so we have the concentration of the faults, the activities spatially related to them, that they are over old features at high angle subsidance. And there is an indication from many different methods that the area is rapidly subsiding at present.

Another area I will briefly mention in south central Connecticut, the Moodus area, will roughly give you an idea of the region we are going to cover, the actual Moodus area, the noises in the center.

When we started the study what was known of the structure of the area, it had been interpreted as a great number of refolded folds and various kinds of sort of taffy tectonics. There have been a great number of field studies going on through here in cooperation with the Connecticut survey and others.

The present major structural features in the area indicate this (Indicating). Crossing roughly north-south through the area is a galomeal brook fault zone which turns out to be a very major early Paleozoic fault zone, possibly pre-Cambrian, but it is a very major fault zone.

The Connecticut Yankee power plant is located, oh, just above the "E" on the Bone Mill, and the recent swarms of activity is extending up to the northeast of that "B." Bone Mill actually passes right alongside the plant. People here are standing on the fault, and the plant is behind. We can see very little evidence of any brittle deformation on this fault zone which has intruded many places.

As an example, the more recent faults we have found in the area -- of course the mapping has been concentrated in the center -- are these little northwest ones and little northeast ones. These are brittle deformations. They are not the older ductile faults. The triastic dike swarm crosses there on the northeast. The circles are the earthquake swarms which John Nevil will speak of. I am giving you a general location.

On those maps there we see the earthquake swarms have this northeast trend along this, just south pretty much of the Salmon River which is indicated to be fault controlled. But there is also all of these great numbers of small northwest offsets where we have looked an investigated. These have turned out to be faults, so there seems to be a relation between intersections of the northwest trends and this northeast feature where these swarms are located.

Now, again, these high angle faults that we have located show a much greater relationship to the activity than the older, more thrust and compressive features.

Another feature that seems to be quite important is the edge of the Cretaceous boundary and the off lap and the irregularities on this. This is just roughly showing. You will see a sort of area of maximum activity in some way down the coast. I+ coincides with the edge of that, and it swings out around the Cape and into the Boston area. There's not anything left to the north, but there is some warping on Pleistocene deltas on the main coast, put together by Woody Thompson, that there is more down more on the coast lying in post-Pleistocene times. That continues on up from where this Cretaceous hinge line ends.

There is also the matter that the activity off Boston in the past has been pretty much on the edge of the Cretaceous, and we have had two earthquakes this summer on the boundary of it at Woods Hole and in the Long Island Sound.

To apply then what we have found in New England and to look elsewhere to see if there is any continuity in the matter of earthquakes in relation to topography, we have then the topography is related to present-day movements upbound. Upland areas are moving up; lowland areas are moving down. And there seems to be a relationship to underlying extensional faults. And that much of the relationships and the earthquake activity is occurring along the coastal areas and the inland St. Lawrence.

Looking at all of the east then, we see the bands, the two principal bands of activity, again from northwest, northeast Arkansas to St. Lawrence, and then along the coast down to the Great Smokies and Charleston. That eastern band breaks down to a coastal belt really and an upland belt.

In general, the activity then in the inland area is really almost all less than 200 meters on bedrock. It is a very low band area. In the central part there is a little filling in.

The other areas in blue here are the real lowland areas of activity, but there is a definite pattern: in the Adirondacks, upland rising, and in the main Appalachians there is a great deal of evidence indicating they are going up in the upland area, and they are going up. These are the areas of evidence of present-day or very recent movements, actually areas that are moving today or in the very recent past, which goes along with evidence of subsidance in the inland areas. This fits not only New England but the rest of the east.

But where we look really on the later activity, in looking at the subsidance we see that the major earthquakes of all the eastern United States occur on irregularities along the Cretaceous coastline, and where that disappears to the north they occur along bays. And the activity in the Mississippi embayment, the southeast Georgia embayment, a little bit in the Salisbury embayment, one at Raritan Bay, they swing off, it goes around in Boston and has no real name, and on the bay to the north of that. That is one major overriding relationship. Then there is the activity down the St. Lawrence.

Now, Bob had mentioned that offshore they could not see deformation off the Charleston area. This is a roughly north-south section along the beach area from the Cape Pier arch down to northern Florida. Recent studies on the shorelines along there by the Skittaway Institute shows a great deal of shoreline deformation, that the tertiary Pleistocene and the recent shorelines are down more, down across the Georgia embayment, up towards the Cape Pier arch. These features appear to be still active. These are at right angles to a lot of the Mezosoic structures in the area. There are a great number of dike systems that extend away from this at right angles to it. Charleston is on the flank of this bowl.

There is evidence, then, for very recent active movement in this region. We look, then, at what may cause this concentration, and we see that these spots in the embayments are underlaid by areas of extensional faulting of different ages, some Mesozoic as to the east, probably Pre-Cambrian elsewhere.

So wherever there seems to be an irregularity in the cretaceous coastline and up into the New England coast, we have these high angle New England faults, probably made a weak area.

Just as an example of this, in the New Madrid area, the work put together here, this is by Dick Sterns, and the work was by Tom Buschback on all the iceograds and the different horizons, and shows a very definite relationship between the Pre-Cambrian graben and what has gone on since, if you draw it on. The red lines outline the graben, and you see here the basal cretaceous surface here warps down right over it.

This apparently controlled a lot of the warping in the late Cretaceous proper orientation and the earthquake activity goes right down that center. We know this was going up and subsiding then from sediments up to early Tertiary, and that section area is still where the Mississippi drains down the center of the continent. There are also several stretches of the river right in the center of the activity in which the bottom of the Mississippi River slopes to the north, not to the south, and many of the engineers who have worked on this problem think there is probably some present day deformation causing this.

In the upland areas of activity it generally is inland from this margin, sort of concentrated here between the Blue Ridge and the valley and ridge, and up in the Adirondacks, a couple of the white mountains, the trend is a weak trend to the northeast, but many of the local concentrations as spots near Charleston and to the east. Local concentrations will tend to go northwest or northeast, but it tends to be in general a weaker amount of activity.

The activity that has occurred farther inland, the faults that had been located in these areas are all ones -- this is just a compilation. They tend to be to the north and northwest extensional faults or purely vertical, where you don't know if they are normal or reverse. But this is a consistent pattern in New England. Wherever detailed mapping has been done, the latest faults -- and we can show this in detail on all of the maps that have been done lately -- are the ones that trend north to northeast and northwest. We find these cutting the Mesozoic structure.

Fracture analysis on the Mesozoic structures will tend to show with sort of a north-south compression, angle of compression which produced these. But this is a very consistent pattern. They are very small faults, for the most part, and in the Lake Champlain, though, we do have evidence of seismic surveys that these may have reactivated and cut the Pleistocene sands. The detailed seismic surveys done in the lake there by Allan Hunt and Jack Dowling of the Universities of Vermont and Connecticut indicate that the lower lake beds of Pleistocene are offset. The Ossippee fault, also in the northwest, may have some movement, another suspect there.

MR. OKRENT: We are running out of time, Mr. Barosh.

MR. BAROSH: I was trying to run over as I thought there was extra.

In summary, then, the activity that we see in New England, and it seems to be also in the northeast, is major activity concentrated along the cretaceous coastline, and then where we lose that boundary, offshore to the north along the bays, any embayments, including that in the St. Lawrence. There is some upland activity next to it and inland activity where it seems to be associated with these northerly to northwest faults.

Putting that in the model as a cause, we would say that the activity is on these coastal plain embayments over extensional fault areas, probably making a weak spot, some in the upland areas, and this further inland lowland activity along these high angle faults. This vertical movement which has been measured we think could be explained by regional extension as some continuing opening of the Atlantic, and perhaps a lot of these small northwest faults can be just landward reflections, perhaps, of these northwest trending fracture zones.

It just appears as if the extensional forces in the central Atlantic continue into the eastern United States.

I will just a show a brief slide or two. This is the New Brunswick earthquake you will see mapped at the center way up there (indicating), which Don Ebel will cover and discuss. That is in an area of great wilderness. It is on a lot of northwest topographic trends and one strong north-northeast topographic trend. It is a normal structure map there.

The area along the main border we investigated, and that is Intensity 5, and there from the last earthquake, so there was minor damage to a number of buildings, put together really by the main survey, and it did occur in an area of previously known activity but it was larger than we expected, and this might indicate we could expect such size earthquakes from central Maine and southern Maine.

Our recent Gaza earthquake in New Hampshire was, again, very widely felt, and there was some damage to houses through that area, plus that in the epicenter, which has been put to center. There are five different towns with one damaged house in the epicenter, and these are all aligned along the northwest trend, whether this has any meaning or not.

And I will close on the note that we just recently found out that there were some fatalities in this earthquake, and we regret to announce that in the Town of Brisbane, New Hampshire, five chickens were killed. On that note I will close.

(Applause.)

MR. OKRENT: Thank you.

Questions or comments?

MR. ZOBACH: My name is Mark Zobach from USGS, and I will have an opportunity to present some data tomorrow. But I think it is appropriate to say at this point that there is overwhelming evidence that compressional stress and compressional tectronics is currently active in the central and eastern U.S., and the deformation on faults which can be shown to be active in Mesozoic time and were extensional in Mesozoic time is now either strike, slip or reverse in nature, and these faults may have introduced weak zones into the crust but they are now deforming to a currently compressional stress field and not an extensional stress field.

MR. BAROSH: We had that information or much of it that you talk about in a paper prior to this study six years ago, and we have been investigating these along with it, and we really found that most of this information was not conclusive, it could be interpreted many different ways, and that what we were finding were actually measurable vertical movements, things you could see.

The other evidence is concentrated on areas where there were earthquakes. If you look into the earthquake areas, the kinds of structures we are finding there, the recent structures we are finding there, and there are orientations, and the fracture analyses and stress work being done on the Mesozoic rocks do not really indicate that, we have found. This is pretty much of an empirical wrapping up of the data.

We have looked at the highway cuts, we have made further investigations in these, and we found a lot of evidence for old strain release from Paleozoic stress fields. It is very difficult to try to separate from recent activity unless you go into purely working on the later Mesozoic rocks.

I would say that the data that we had put together or found in all of these various studies definitely show there are vertical movements going on; that these vertical movements pertain to extension, the high angle extensional faults, particularly from the New Madrid region up the St. Lawrence. You can see a lot of these faults along the St. Lawrence that could have moved yesterday. There are quite brittle features on them.

They are just properly aligned parallel to the Atlantic, and we think that the Atlantic is still opening a little bit. We are still getting a down-bowing along the cretaceous boundary, but only in the spots where there are irregularities, these underlying extensional faults, and where the surface rocks more or less are bowing down over them.

MR. ZOBACH: Well, I have made my statement and I will try to support it tomorrow.

MR. BAROSH: We started with similar data. Ten years ago we had compiled the faults on the Atlantic coastal plains and started looking that way.

MR. JACKSON: Have you done any statistical correlations between the lowlands and the seismicity or is this a general observation? I am trying to get a feel for how you have reached this conclusion of this essentially univarient relationship.

MR. BAROSH: It is a general observation. We went out and checked this. It was something that came up because one of the things is the geomorphology in New England very closely reflects geologic structure. It was sort of the homeland where geomorphology began in the U.S. We can see almost every feature in the geomorphology reflects some geologic feature, and then we saw that seismicity is almost all concentrated in areas, and specifically around Passama Quoddy. You can draw this 300 meter area boundary, and that encloses all of the seismicity, and then to realize that activity down the St. Lawrence to the New Madrid area is almost all under 200 meters elevation of bedrock.

And then when it turned out the surface movements were occurring and the measurable surface movements were up in the few upland areas and down in the lowland areas, and with the knowledge that the geomorphology is closely controlled by structure, this gave us a good clue to work on.

I think we have the data now for seismotectonic zoning in New England, and the problem is amalgamating somehow the data on the seismicity which will show a cluster of activity in the area plus the boundary that we can get from vertical movements plus putting geologic block boundaries on this. In some cases these aren't quite in line, but this is what we need to juggle. But we know the data now that we need for the zoning.

MR. OKRENT: I think we had better hear now from Mr. Anderson, and we will have a break after the discussion on his talk.

MR. ANDERSON: Thank you.

I have been asked to give a short presentation on the tectonics of Maine with some discussion of the Norumbega fault in 15 minutes. This reminds me of my college days, the examinations, the geology of the world in 25 words or less.

MR. OKRENT: You have my apologies, that is all I can say.

MR. ANDERSON: Yes. I understand everyone is in the same boat.

There is a colored slide on the screen which is a very generalized map, a geologic map of the State of Maine, and the Norumbega fault system is this (indicating). You can see the dark line. This represents what we call the Norumbega fault in Maine. It was named by Professor David Wons at VPI. We have done some work up in this area on that system, and I would like to emphasize that it is not the Norumbega fault, it is a system, and it is made up of many parasitic faults, associated fracture systems and what have you, but pretty well closely spaced and pretty well identified as perhaps one large single unit.

It extends for approximately 200 or 300 miles from the New Hampshire border to New Brunswick. It extends principally through lower mid-Paleozoic crystalline rocks. It is the principal movement along that fault, which is identified by Wons and other as right lateral.

The characteristic phenomenon that identifies it is mylonitization, brecciation and in some cases slickenside, silicification zones aplenty, offset of metamorphic osograds, especially down here in southern Maine. For those of you familiar with Maine, of course there is an increasingly metamorphic grade as you go towards southern Maine. You get into second solenites, dirt ray rocks up in Maine, offset isograds, benolic isograds.

One of the identifying characteristics of that fault is we have differing structural styles often dramatically and abruptly occurring on both sides of the fault, the juxtaposition of different stratographic groups. There are lineaments and straight stream segments associated with that lineament, and also recently, based on some of the work that we have been doing with the Maine geological survey, we have been able to demonstrate that there are high yield groundwater wells which can be contoured along that fault zone.

So it is what you might call an important bedrock aquifer, and we are able to actually contour -- I will show you a few slides of it later on -- what we might want to call groundwater lineaments, which also help identify the extent of that fault and the faults parasitic to it. There are many faults that are parasitic to it that come down off of the main fault down to Penobscot Bay, down through the midcoast area here. Many of the estuaries we see in this area, the structural basis for those estuaries are the parasitic faults extending off the main fault zone, if you will.

Those are some of the characteristics that have been identified as far as identifying that fault is concerned. The sequence of the tectonic events associated with it is usually we have had -- of course, the first major deformation associated with the Paleozoic was folding and then metamorphism that was introduced by the Arcadian. Then we had major horizontal or stretch/slip movement of the Norumbega fault.

Then we had the emplacement of plutons. The red you see on the map are plutonic bodies. We had an emplacement of plutons. Then we had what was apparently a reactivation of that fault, which was expressed in vertical rather than a horizontal component and could possibly be related to the triassic rifting. I won't get into the arguments I can see will develop on this one.

We have some plutons in southern Maine, the Lyman pluton, the Saco pluton, which have been dated as upper Paleozoic, that are associated with this major fault zone, and I would suggest that then as we go up through time, we have had the horizontal component. We have had the vertical component during the late Paleozoic or even perhaps Cretaceous.

Now the question is let's go further up into time into what might be a possibly younger movement along that fault zone. Of course, one of our programs with the NRC was to develop, to look at the Quaternary up in this area to determine whether there is any recent displacement.

I am sure, as you all know, the work that was done here at Sears Island several years ago identified Quaternary sediments which were apparently placed over a major fault zone. It was parasitic to the Norumbega, which perhaps date movement somewhere between 50 and 30,000 years old, and it was the judgment of the NRC, I believe, at the time that that fault is capable, and I guess the current status is that that cover has been cancelled.

David Wons and Woodrow Thompson have done some work in this area looking at possible recent movement along that fault zone. They did find some what is apparently some minor displacements, but the results are really inconclusive as to whether there has been any recent movement along that fault zone but we are continuing to look at this area. We would like to continue to do so because I think we are at a critical point in terms of understanding this system, not only in terms of tectonics but also in terms of recent movement.

Now, I have here a report that we have just put together. I will leave two copies with the Commission and I will submit the rest of them to Tom Smith, the other 26 or 50, whatever it is, that we owe them at the end of the meeting. This outlines the summary of the 1981 program, and for any of you interested in the details of what I am talking about, you can find them in that report.

I would like to point out that the entire effort here, if I can move on, this (indicating) is a part of the Norumbega fault system passing down through the central coastal area of Maine, and these red areas that you see -- and this is the only slide I could find. There are others more dramatic than this. You can see that there are high yield areas -- by high yield I mean 50 gallons a minute or more -- that seem to line up along those fault zones.

So in some cases we can't see the fault zone because of considerable overburden, the contrary, high yield wells, utilizing drilled wells which more or less follow the continuity of that fault zone. That is just an example of how some of the programs going on in the Maine Geological Survey have fit or overlapped and also enhanced the NRC effort in this area.

This is the State of Maine going the wrong way. That is East and this is West. It is backwards. But this is Bangor, this is Callas, and Eastport going this direction, and these are the national survey loops. I think Pat had a slide that referred to this. We have developed comparative profiling along some of these loops to determine whether or not there was any subsidence or any movement, crustal movement going on.

This was in collaboration with the Department of Civil Engineering of the University of Maine. This is Bangor and that is Eastport, Bangor to Callas (indicating.) This is 1942, 1953, and the latest is 1966. Within a period of approximately 24 years, we have had the subsidence of a little over half a foot, and by taking the comparative profiles and developing a vertical surface map, we are able to develop a surface map -- and Pat had one of these slides -- approximately 9 millimeters a year, which calculates to 3 feet a century, which indicates there is some dramatic subsidence going on in this area.

Also in this area (indicating), here is a warp here (indicating), so this information also piqued our curiosity as to whether or not there is a strain field going on up in this area. We would like to also integrate this information with the tide gauge information of Stacy Hicks as we have a large volume of information we put together, but we would like to integrate this map with the tide gauge information to bring this into a total picture.

One of the other elements we brought into play here, I might point out, is that this entire effort has been one of multidisciplinary effort. It involved geologists, seismologists, historians, archaeologists, civil engineers, you name it, they have been involved in it, and enthusiastically involved in it, I might add. And all of them have generated some very interesting information.

I don't know whether you can see it on this slide or not. Can you see this little ridge which takes off in the salt marsh? You can see it going around there, and another right here. Can you see it (indicating)? These are salt marsh dikes. These were built during the late 1700s. This is another one of the Colonial -- I think Pat showed you a colonial structure that was buried under about 3 feet of salt water peat, which reinforced the sea level rise or subsidence, if you will.

This is another feature. This happens to be a Colonial feature very well documented, historically documented. It will be described in the reports I have handed to the committee. But the salt water dikes were in place in the late 1700s or early 1700s to keep out the tidal waters. The salt marsh has grown back behind those dikes, and it was a very valuable economic product in those days. Very little of the land was cleared and they used it for feeding their cattle. A lot of it was shipped to Boston and it was a very important economic base along the Maine coast or in Maine during those days, and those dikes are still in place.

Taking cross-sections through them and comparing the buildup of peat on the seaward side as opposed to that on the other side of the dike, you can get a pretty good idea as to what the rate of peat buildup is on the seaward side, and hence the amount of subsidence, if you will, or sea level rise, if you will, that has taken place since that time. So we have a time marker in this, and it again confirms about 3 feet per century.

This is just a couple of curves. You have probably all seen these. But these are the ones taken at the primary stations in Eastport and Portland, Maine. We have also gathered information from the archives, from the ocean survey that develop additional curves to enhance that velocity surface map that I showed you earlier. So there is another element that we have brought into this thing.

This shaded area in here is essentially the limit of the marine transgression right after the last glaciation in Maine. Within this area we have been looking at the elevations of deltas that were deposited off the ice front into post-glacial sea, if you will, prior to the glacial rebound, and there are a number of deltas which occur in this area and down here (indicating). We are very careful in measuring the elevations of the top sets and the distributory channels in places where we can see them carefully, leveling them with the help of the engineers.

Here is a typical example. You can see this is a classic topset, foreset, bottomset relationship. In some cases if you get up on the surface you can actually see the distributory channels, inter-tidal channels on the surface, so you get a pretty good idea pretty close as to where sea level was, and to actually survey them in, and by mapping those elevations we are hopeful that we can determine any abrupt breaks or variations in elevation.

Last summer we had Dr. Bjorn Anderson from the University of Oslow who was doing some detailed work up in this area, and he noted a rather abrupt change in elevation of those delta surfaces along a line right above here (indicating). That abrupt change was something on the order of 60 feet of change. What that means I am not sure. It extended along a line, a northwest-southeast line. Everything on this side of the line was at the higher elevation, and then a 60 feet drop in elevation on the east side of the line.

We are putting together the information now. We will be submitting that in report form very shortly to the program managers of the NRC.

MR. OKRENT: Mr. Anderson, you will have to finish in a minute or two.

MR. ANDERSON: All right. I will shut it down, then.

I would like to reemphasize the fact that we have had this interdisciplinary collaboration in developing this program and we have worked very closely with the Western Observatory and the seismologists, and I think the effort, if we are going to gain a further understanding of what is going on in this area and understand the tectonics, is to enhance that interdisciplinary approach, especially between the geologists, the geologic effort and the seismological effort.

I would also like to point out all programs of the Maine Geological Survey have been integrated. One of the common threads going through all of our programs is good geologic mapping, and that the NRC has benefitted from those programs.

Are there any questions?

MR. OKRENT: Mr. Philbrick.

MR. PHILBRICK: Did you state the Norumbega fault was capable or uncapable?

MR. ANDERSON: I didn't state it. I have said there is a fault that we believe is parasitic to the Norumbega fault which extends through Sears Island out into the Passama Quoddy Bay. There was a trenching effort that went on there back in '76 or '77, I believe, across Sears Island. I didn't see this myself, I was in the Antarctic at the time, so I didn't have a chance to look at it; but based upon the descriptions and the slides I have seen, the sediments overlying that ancient fault were displaced, apparently.

MR. PHILBRICK: I had a pleasant time on the 1st and 2nd of August of last year on the field trip run by the Maine Geological Society, at which were present Dr. Hussy and Dr. Westerman.

MR. ANDERSON: Yes.

MR. PHILBRICK: And I went to find out from them what they thought about this fault, and with a direct question to each of those men individually of is it capable, is it active, as Dave said in his article, and they came back and said they didn't think it was.

MR. ANDERSON: Well, we don't know.

MR. PHILBRICK: The fellows mapping in the field.

MR. ANDERSON: Pardon?

MR. PHILBRICK: The fellows mapping in the field, Hussy and Westerbrook.

MR. ANDERSON: Yes, yes.

MR. PHILBRICK: Both of them had a negative approach to that.

MR. ANDERSON: Negative approach? Well, my dealings with those people, we just flat out don't know whether the thing is capable or not. The only indications we have had that it was capable was the one incident on Sears Island, and that wasn't a part of the principal Norumbega fault zone.

MR. PHILBRICK: What you have just said, the only incident you have for activity on the Norumbega fault zone is a fault which does not strike parallel to the fault, which is not necessarily tied to it by direct contact mapping, by one line to the other, that there is no motion shown on the Norumbega zone itself which shows activity.

MR. ANDERSON: That is right. That is right.

MR. PHILBRICK: So then, for the record, the main strand of the Norumbega has no evidence of activity sufficient to indicate that it is capable. That is all negative at the present time.

MR. ANDERSON: That is right.

MR. PHILBRICK: Thank you, sir.

MR. ANDERSON: I would also point out, though, that there is no question in my mind that we do have subsidence, a considerable amount of subsidence going along on the Maine coast, a considerable amount, one that invokes some concern. The question that comes to my mind is is there a strain field of some sort developing there, and we have furnished considerable evidence for that.

We have an historic record in our old quarries, not so old, the late twenties or thirties, of rock bursts when that industry was in action, where large granite blocks as they are being quarried away from quarry walls in some of those quarries literally exploded. They were under some sort of stress.

Now, whether that was the result of the emplacement of that rock or something else, I don't know. This is what we would love dearly to find out. I know the USGS is intrigued enough with that concept that they have begun a program and will be in next year doing some strain field analysis up there. Drs. Fitzhugh and Lee from Denver will be in. They will be doing some stress analysis on some of the lithologies in the vicinities of that Norumbega fault zone.

There is a body of evidence there that suggests to me, or at least I have some concern about it as far as any kind of heavy industry or heavy siting is concerned in that area, whether it is new plants, dams, pipelines or what have you, and while admittedly that is true, we don't know what is happening on the Norumbega, we know it is a big system but we do know that there is vertical movement going on there. I mean subsidence.

MR. PHILBRICK: Let me go in for a minute, if I may.

MR. OKRENT: I will give you 30 seconds.

MR. PHILBRICK: More than that. You have no information that shows that in the central part of the coast, in the Bath areas, that there is any indication of any present activity in that fault.

MR. ANDERSON: No, no.

MR. PHILBRICK: That is correct, then.

MR. ANDERSON: I never said that.

MR. PHILBRICK: I didn't ask if you said it; I am just asking about evidence. You have given me a negative answer on that all the way through.

MR. ANDERSON: That is right.

MR. PHILBRICK: Correct. It is a pleasure to do business with you.

MR. ANDERSON: Any time.

MR. PHILBRICK: I pay my license fee up there and my dues, and I did my doctoral thesis up there, too.

MR. ANDERSON: Excellent.

MR. BROCKMAN: Steve Brockman of the NRC. I just have one question. Several times during your talk you said the subsidence was three feet for a century. I was wondering if you took into account the general rise of sea level.

MR. ANDERSON: Yes. I think we know, at least in the last 100 years, the eustatic sea level rise has been something to the order of six inches, so you can subtract that, if you wish.

MR. OKRENT: All right. I think Dr. Savio has a brief announcement before we make our break.

MR. SAVIO: If I could call your attention to the first announcement on the agenda, the hotel will be selling tickets to their luncheon buffet in the hallway during the break. It is not the only way to get lunch, but I understand it is about the fastest.

MR. OKRENT: All right. We will take ten minutes.

Let me see. The next speaker is Mr. Ebel. Is he here? Will you be back, please, in ten minutes, and I will and the recorder will, and we will begin.

(Recess.)

MR. OKRENT: Is Mr. Ebel here.

MR. EBEL: I am John Ebel from Western

Observatory, Boston College, and I am talking about seismicity of New England.

The first figure I have is just a configuration of the seismic network run by Western Observatory. We have 36 stations, all of which are presently recording the data. Telemeter 2 is presently being recorded on developed quarters. The station spacing is typically 50 to 100 kilometers, except for two micro-earthquake networks, one up here in the Dickey area of northern Maine, and we have had equipment problems up there so all of the stations in that network are down.

We have another micro-earthquake network down here in the Moodus Haddum area, whih recorded the earthquake swarm in Connecticut last summer and fall, and I will be talking more about that later. This network is presently operating and we have part of the micro-network that will be completed up here in the Passama Quoddy Bay network of Maine.

The seismicity since 1975, this is through June 1980, which has been recorded by the NEUSSN, which is the Northeastern U.S. Seismic Network, is shown in this figure right here (indicating). In general, the threshold of magnitudes is around two for this entire area, and in some local places where the density of stations is greater, the threshold of locations is somewhat smaller.

You can see concentrations of activity here (indicating), here (indicating). La Malle Bay is by far the most active in the Northeast. The seismic activity here is biased a bit by the fact that there is a large number of stations in New York and New Jersey and around Indian Point. Passama Quoddy Bay shows up. There are a few earthquakes up in New Brunswick, and you will notice this earthquake right here, that is in the same general area as the earthquake that occurred on January 9th of this year. I will also talk a little bit more about this earthquake later.

If I could have the first transparency, it is interesting to compare this pattern of activity since 1975 to the historic seismic activity which has been documented. The figure I want to show here is the seismic activity from the catalogue at Chiburis. There is some activity here along Hal Bay. It shows up very strongly in this area, the Moodus area in Connecticut down here (indicating), much activity up here (indicating), and activity down here (indicating). The only area which shows up on this figure which didn't on the last slide is probably this Cape Ann area in eastern Massachusetts.

Since 1975, if you can put on the next Vu-graph, we have had an increasingly accurate count of earthquakes in New England proper. This shows earthquake activity from the inception of the NEUSSN in 1975 to this is 1978 here, and the network was being installed in New England during this time period in 1975, '76 and '77. It was essentially complete at the beginning of 1978.

You can see that the seismic activity recorded by the network has increased somewhat because we were able to detect and locate smaller magnitude earthquakes. If you can put on the next transparency, this will show the activity from 1978 through the beginning of 1981, and you can see that there is a higher general level of activity, one, two, three, four, five, six, seven being the maximum number of events recorded in any one month. On the average right now we are recording about three earthquakes per month in New England, and one earthquake every two to three months is being felt.

The largest earthquake that occurred during the time span from 1975 through the middle of 1981 was in New England proper in Bath, Maine in April of 1979, and it was a magnitude of approximately 4.0. In general the magnitudes represented by this transparency are in the range of 2 to 3, with the largest being this 4.0.

For the area outside of New England proper, Canada and then the New York-New Jersey region, another five to six earthquakes per month are located and approximately one earthquake per month in that area is felt.

If I could have the next Vu-graph.

The earthquake which occurred in New Brunswick on January 9th -- if you could slide it over so we can see the left-hand scale, because that is of interest -- was detected all across New England, of course, and we detected a large number of aftershocks and some foreshocks on our stations in Maine. This is a plot of the number of shocks per hour detected on our stations in northern Maine, 10, 20, 30, et cetera from the beginning of the Maine shock. These should not be MCs. These are local magnitudes.

The main shock appeared here. You can see the seismic activity. The aftershock activity died quickly except for one small burst after the main earthquake, three or four hours later. You can see the activity stayed at the same level until the occurrence of the 5.5 aftershock on Monday afternoon, then there was a tremendous burst of aftershock activity which decayed much more slowly than the activity down.

And in fact, as of yesterday morning we were still recording on our stations in Maine about 10 to 20 aftershocks per day, and our closest station in Maine is about 130 kilometers from the epicenters. Our detection threshold is around magnitude 1.5. So this shows the aftershock activity and the very, very dramatic increase in the number of aftershocks after this magnitude 5.5 event.

If I could have the next Vu-graph, please.

This shows for the first three or so days of the sequence a histogram of the event sizes or the distribution of event sizes with number. You can see that in Maine we recorded over 20, about 25 events in the magnitude of 2 to 2.5, and we had some events up to about magnitude 4. Since then, several more events in the magnitude between 3 and 4 have occurred, and there have been a couple of aftershocks which have been a little bit larger. Then, of course, there were the three larger events that occurred, the 5.8, the 5.1 aftershock three hours later, and then the 5.5 event three days later. This is a summary of what we know at this point of the distribution of events from the New Brunswick earthquake.

On January 19 or the evening of January 18 local time, we had a 4.8 magnitude event in central New Hampshire -- if you can put up the next Vu-graph. This is from topographic maps of the area on Lake Winnesquam, right here. Lake Winnepassaki will be to the right of this figure. Concord, New Hampshire is south about 20 or 30 miles.

The location we got for the main shock was in this general area here. M.I.T. computed a location for the main shock which was right over here. Within six hours of the occurrence of the magnitude 4.8 shock, some portable instruments were deployed, one here, one here, one here. Western Geophysical employed an instrument down here in Franklin. M.I.T. deployed an instrument down here in Tilton. Lamont-Doherty installed some instruments later, and M.I.T. has since installed some more instruments.

From these we recorded several aftershocks. The location of one of the aftershocks was right here, which is virtually identical, as you can see, to the location of the main shock. Control on depth is somewhat confusing. The closest station to the main shock that recorded it was one of the M.I.T. stations, which is 30 kilometers to the north and a little bit west, and using their data and the data from the Western Observatory network, M.I.T. and Western calculated a depth for the main shock somewhere between 7 and 10 kilometers. However, S minus P times at the portable station that was installed right here for the aftershocks which were recorded are about .3 second, which would suggest focal depth no more than a kilometer or so. So we have some discrepancy at this point between the focal depth of the main shock and the focal depth of the aftershocks for the local array.

I have put a few numbers on here to show very crudely from intensity questionnaires which were sent in to us the felt intensity of this main shock in the towns around this area. There were some intensity 5's and perhaps intensity 6's. I would rate them at intensity 5 plusses in the Sanfordton area.

There were some intensity 4 and a few intensity 5 measurements perhaps from down in here. There were intensity 4 and 5 down in the Franklin area, and in fact the U.S. Army Corps of Engineers had a strong motion instrument on a dam in the Franklin Falls area. In fact, I think this right here is the Franklin Falls Dam, which did trigger on this magnitude 4.8 event as well as several stations farther away that triggered.

The distance from here to here (indicating) is about 4 kilometers, so I would say there were within 6 to 8 kilometers of the epicenter of this event. We are still compiling the intensity data. We are still recording the aftershocks. Yesterday at noon right as I was leaving Western Observatory we recorded a magnitude 2.8 aftershock, which our location put it in the same general area, and that was felt in the towns around here (indicating).

If I could go back to the slides -- I am sorry, let me hold off on the slides for one moment.

We did record some prior seismicity to both the New Brunswick earthquake and the New Hampshire earthquake which is of interest. The event I pointed out on the seismicity slide, which was September 1, 1977, located in New Brunswick, looking at S minus P times of our stations in Maine, it looks as if this event was locates somewhat to the southwest of where the main shock epicenter was.

There was an event on November 28, 1981, so about 2-1/2 months ago, which -- I am sorry, a month and a half ago -- which located in virtually the identical place as this event right here, and the S minus P time set up from our stations in Maine are within half a second of those from some of the aftershocks.

We also recorded some foreshock activity in the day prior to this January 9th event. There was a very, very small event of which we just got a trace of 2107 on January 8. There was perhaps an event at 2349. We couldn't tell for sure whether or not the wiggles were an event or not. We did record traces of two very, very small events which occurred within 10 or so seconds of each other at 0405. There was very definitely a foreshock at 1212, which was at about the limit of our measuring, which was 1.5. And then, of course, the 5.8 main shock at 1253.

For the New Hampshire earthquake there were large numbers of events that have been recorded in the general area. This is all epicenters within 25 or so kilometers of the event on January 19. It is of interest that on June 28, 1981 at 2242 there was an event in virtually the identical spot which was magnitude 3.0, which was felt in the area. M.I.T. installed some portable instruments for a couple of days after this particular earthquake, and as far as I know, they did not pick up any aftershocks at all.

We have only recorded a few aftershocks on the portables from this event to yesterday when we got the magnitude 2.8 event. When I am talking about small, I am talking about events so small they can barely be seen on the MEQ records.

Now if I can go to the slides, I will talk a little bit about the swarm of activities that was recorded in the Moodus, Connecticut area last summer. Moodus is right down here, and this is a blowup of the seismic activity from the Chiburis Catalog, and I show it mostly to locate Moodus in the state of Connecticut, and there are are a large number of dots, circles and squares in that location, the location of the earthquake. This is a generalized geology slide of the one that Pat Barosh showed earlier. The Bone Mill Brook fault is here, the Salmon River with its inferred fault is here. Most of the swarm, including the magnitude 2.1 event, occurred at this point here on August 4th (indicating). All the activity we recorded after August 4th until August 18th was centered right here.

On August 18th we picked up an event here, on August 21st an event here, and in September we picked up some more events down in this general region. Our location error is about 1 kilometer or a little less for these events, mostly because they are so small and not very well recorded on many of the stations. Our micro-earthquake net here is located here, here, here, here and here (indicating), so we were able to surround these epicenters with at least three stations and then several other stations in the immediate vicinity.

This is a time histogram of the numbers of events per day recorded during the swarm. There were some foreshocks recorded to this event also to the magnitude of 2.1. About 44 shocks occurred on the day prior to the event. The activity subsequently modulated in intensity except for these later bursts of activity. These arrows show the times at which we picked up events from the new locations.

This is a histogram of the energy released for six hours during the earthquake sequence through the middle of October, and you can see that at this place here and this place here (indicating) when we detected new event locations we detected somewhat larger events. When I am talking about larger events I am talking about events of the order of magnitude 1. I should say all of the events were shallow S minus P times. A portable instrument, an MEQ 800 installed in this region showed S minus P times of 1.5 to .2 second, and all of our locations put the events no more than 1 kilometer oir so deep.

On August 3rd it looks like one of the foreshocks was heard. The August 4th event was heard and felt by people in the area. Some of the aftershocks were also perhaps heard and felt. This is very interesting. People reported hearing booms during the night although the largest aftershock we recorded was magnitude minus 1 or so. We convinced some people to keep accurate records of when they heard or felt something, so we were able to confirm when some of these later events occurred that in fact the events had been heard or felt, and you can see that events down to magnitude zero or so were being heard, and events of the order of magnitude 1 were being felt by people in the epicentral area, and I am talking about within 2 or 3 kilometers of where the earthquakes were occurring.

MR. OKRENT: We only have a couple of minutes, Mr. Ebel.

MR. EBEL: The event on August 18th was heard by a woman living between the epicenters of the August 4th and 18th events, and she in fact told us before we told where it was located that this event had come from the opposite direction of her house from where the other events had come from. Having looked back at records of some old earthquakes from the Moodus area -- this does not show it very well. These are Bennioff records of earthquakes recorded at Western from the Moodus area.

The August 4th earthquake shows up here, and a Rayleigh wave showed up for this earthquake and it also showed up for two which occurred in 1940 and a little less so for one occurring in 1968. We don't have a record for 1951 from an earthquake from this area, but just looking at this data I would say that probably these earthquakes were shallow also.

I think that is all I have.

(Applause.)

MR. CHINNERY: Chinnery, M.I.T.

Tell me, how did the New Hampshire earthquake compare with the location of the 1940 earthquakes, and how did those compare with the Ossippee Mountain structure?

MR. EBEL: The 1930 earthquake sequence was at 20 or so kilometers to the northwest. Now, I understand that that location, at least from the word I have from Bob Linahan, that location was made based upon intensity reports and just a few S minus P times, so I don't know how accurate that location is; but these earthquakes are definitely to the southwest of the 1040 earthquakes, and then the Ossippee plutonic complex you were talking about is to the northeast of where these occurred.

MR. CHINNERY: By how much?

MR. EBEL: I think 10 kilometers or so, just from going through the "felt" reports we have, and Pat Varage talking to people in the area who also observed this. People living in that complex didn't feel nearly as strong shaking as people living on the other side of Lake Winnesquam.

MR. HARPER: Harper, NRC.

Do you have any word as to when we may have the word from the Franklin Dam?

MR. EBEL: I talked to Ed Black yesterday morning and he said a crew from Vicksburg is in the area right now. They are changing the records, they are calibrating all of the instruments, and they hope to get that work done this week, and they are going to make copies and distribute them. So I would say it will be a couple of weeks.

MR. OKRENT: All right. I think we will hear next from Mr. Grow.

MR. GROW: Let me have the first slide.

What I will be showing you this morning will be marine seismic reflection data which was collected last September south of Long Island and east of New Jersey. In that recent survey we detected a fault south of Long Island approximately 20 miles east of Sandy Hook, New Jersey where the Port Hancock well is. This is the first time we have had any reflection work into this part of this body of water here.

I should say that the body of water here east of New Jersey and South of Long Island is referred to as the New York Bite by mariners, and we have termed this fault the New York Bite fault.

Before I get into the details of the reflection, let me give you a little bit of regional background on the offshore geology and what we refer to as the Baltimore Canyon Trough. This is the tectonic map of the Western North Atlantic and Eastern North America. The area where the fault was detected is just south of Long Island here (indicating) and just northwest of a deepset entry basi. shown in orange, which we call the Baltimore Canyon Trough. Just seaward of that underneath the present continental slope, you get into what we believe is oceanic crust, and these northwest trends here are fracture zones in the Western North Atlantic zone crust, and these northeast lineations here are magnetic lineations of Mesozoic age.

Next let's look at the depth evasement contour map in the Baltimore Canyon Trough area. This runs along the margin. Here is Long Island shown in white here. There is Raritan Bay, Sandy Hook, the Cape May, New Jersey and Delaware Bay. The deepest set of entry deposits in the Baltimore Canyon Trough is in excess of 12 kilometers depth underneath the present continental shelf. They also extend in excess of 10 kilometers of depth under the continental rise.

The tan defines the sediment thicknesses less than 2 kilometers in thickness, and the area the fault was detected in is right up in here where we actually have less than 1 kilometer of sediment. The survey we were actually working on where we were trying to look at these triassic grabens through here, what we were actually looking for was to try to define these better, but in fact of importance to this particular meeting this morning would be a fault we detected in this area (indicating) right there.

What I will be showing you will be five new reflection profiles we collected in September, and they include this line 2, 3, 11, 13 and 16. We had one previous multichannel reflection line in the area at line 24. I will show you that also. We had noticed a little anomaly on the basement of that area, but because that was collected with a 3300 meter streamer of 40 channels with a 30 ergon array, the resolution wasn't adequate to make much of a case for a fault on that particular line.

It wasn't until we shot that particular line coming through here with a higher resolution reflection system that we became aware of the fact that we had probably picked up a fault. Then we in fact modified our cruise during that time to pick up additional lines through here using some single-channel data. The reason we had to go to single-channel data through here is because we are in the middle of a shipping lane in New York Harbor and we were dodging traffic most of that time. The lines through here were also recorded single channel but the records were in fact quite good.

What I will start with will be to show you the southernmost line and work to the north. This is line number 3. I am afraid the label is off the top of the screen. Were are looking at a reflection time from zero tc one second here. This is a single channel -- I am sorry, this is the near choice monitor record off of a six-channel streamer. This was shot with two 80-inch water guns, and the data has not been processed. It frequency band for recording on the monitor records is about 8 to 60 hertz. We have a basement event down at close to .9 of a second.

Our estimate at this time was about 30 milliseconds of offset on the basement at the southern end of this, and the velocities of sediments in this area are close to 2 kilometers per second for the sediment, so that 30 milliseconds is approximately 30 meters of offset if the correlation we made here is correct. There also seem to be shallower offsets through here all the way to somewhere between 100 and 200 milliseconds. We still have an offset here.

These first three pulses here are direct rivals from the sound source to the receiver system rather than a reflection, so we can't resolve anything through here.

Moving to the next line to the north.

This is our older off-the-channel line, using a 36-millimeter streamer. Looking back on that, we see an offset on basement, but because this data was processed between 15 and 45 Hz, and because of the long streamer and the big offsets between the sound source and the receiving, we do not have a whole lot of resolution through here. But we do see an offset on the basement here. And at this particular area here, we estimate about 75 milliseconds of offset.

Moving farther to the north, this is again a single-channel monitor record from a 600-meter streamer.

On this we see something like 65 milliseconds of offset on the basement here and quite shallow deformation going up to the point where we get tangled up in the direct arrival from the sound source, as much as 40 milliseconds offset in the shallow section here. This again was recorded analog data between 8 and 60 Hz. We have not had a chance to process the data yet, but we hope to have that soon.

This is a single-channel line which has been processed with filtering between 20 and 110 Hz. It has also been refiltered. In this particular area we see an offset of about 85 milliseconds on a basement in here, and it continues right up to the point where we may have offsets of even 40 milliseconds up to a fairly shallow area in this section. The direct arrival obscures the sea floor itself.

This is the next line to the north. This is again single-channel digital processing. We have a good strong reflection of basement in through here with about 75 milliseconds of offset here, and then it decreases. There is still some indication of shallow.

The northernmost line, the last line, this is still single-channel monitor data, and as best we can tell it does not have any offsets affecting the basement or the overlying sedimentary section. So it appears that the fault does terminate somewhere south of Long Island.

This is a geologic map with the quaternary deposits removed. I would point out Long Island is underlain by layers of cretaceous sediments in through here. The Triassic, the Newark is in through here in brown. The Ramacle fault is shown over here, and the Pre-Cambrian and Paleozoic Appalachian deposits are in red.

There appears to be some early tertiary sedimentary deposits coming in, mapped onshore here and probably extending offshore into the area in through here, and further south somewhere we probably would pick up some late tertiary sediments coming in at the southern end through here.

What I will show you next is data from two wells from the Island Beach well and the Fire Island well, and we will show a cross-section between those two wells and give you an idea of how the stratigraphy projects into the fault as we have mapped it at the present time.

I should say the quaternary deposits up here to be less than 100 meters thick in this area, in some cases they have been measured in holes on the southern edge of Long Island, as low as, as thin as 7 meters. Other estimates range from 50 to 100 meters. There is no adequate map of the thickness of the quarternary sediments in this area.

At the Island Beach well we have something like 350 meters of lower tertiary deposits in through here, and those are completely pinched out by the time they get to the Fire Island well. And we have got cretaceous sediment right under quaternary and Fire Island. So we do have upper cretaceous deposits under most of the sedimentary segments through here.

The projections of this cross-section would actually cross the fault at lines 3, 24, and 22. So we guess at the present time that the sediments offset near the surface of the reflection profiles on lines 3, 24, and 2 do include lower tertiary deposits being offset.

In the northern area the projection does not actually cross these lines 11, 13, and 16. But there may be a little bit of lower tertiary deposits through there.

We have one high-resolution sonar record along line 11 which does suggest that there may be deformation within 10 meters of the sea floor. The sea floor itself is smooth. This particular sonar system is a 3.5 kHz sonary system, and sometimes you can get penetrations of upwards of 50 meters with such a system.

There is a warp end that lies right over the trace of the fault we saw in line 11. And it suggests there may be deformation within 10 meters of the sea floor.

In general, this is a pretty sandy area, and we did not get high-quality sonar records in this area with the exception this is the one case where we did see something in the upper 10 or 20 meters.

In terms of the location of this fault, in terms of other aspects of the geology in this particular area, one of the things it seems to correlate with is a gravity anomaly which runs along the inner shelf in this area. In fact, this gravity anomaly lies just offshore. There is a strong gravity low right along Sandy Hook itself. As you go into Raritan Bay there is a strong positive gravity anomaly through here, and this goes into Staten Island where there are in fact mapped serpentine kenites on the northern part of Staten Island.

This goes west into the Bouguer gravity associated with the Appalachian origin itself. I would like to point out these gravity highs trace all along the Piedmont, and this is the only place where these particular gravity highs swing offshore.

Let me show you a gravity map of the continental shelf in this area to give you the regional context. This is Sandy Hook. This is Delaware Bay. Here is Cape May. Here is Sandy Hook, and here is Long Island coming in through here.

The old line I showed you was 24 through here. The fault was detected right along the axis of this gravity positive in through here. This positive continues on up through Teconnick and Green Mountains to the north and then swings westward across New Jersey and continues down along the Piedmont.

In addition to the serpentine kenites that have been found in Staten Island, there are other evidences of pillow basalts and amphibilites which in a general way correlate with the Appalachian suture zone. For instance, Williams in 1917 mapped them in his tectonic map of the Appalachians, and this gravity high has an association with the early Paleozoic suture zone and in a general way with certain Ophealite complexes.

This is the old Willard and Jostane Bouguer gravity map of eastern North America, and we have just covered it here to indicate that if we followed that gravity high -- we did not show any of the offshore data here -- here is Long Island.

The part we are looking at is the only place where this Piedmont gravity high actually swings offshore. So the association of that fault with that particular gravity trend is certainly of interest to us.

Well, coming back and trying to summarize briefly, what we have found as we have gone from south to north is the southernmost line where we actually picked up the fault was on line number 3. We have approximately 30 meters of offset at that point. It appears to go further south through here.

This was another line collected on an earlier cruise this summer, but it was collected with some 500 cubic inch airguns, and at least on the monitor data, which is all we have at the moment, we do not see any evidence for the fault through here.

But that particular system is much lower frequency resolution and may not be adequate to resolve a fault in this area.

Again to the north we have approximately 55 meters of offset on line 24 in through here, and we have up to 85 meters of offset at this particular point here, and line 11, which is where the maximum offset appears to be. And then it appears to decrease again until we get apparently zero offset in this particular area up in through here.

We only have one line terminated on the north. It would probably be advisable to try to get a few more lines in there and see the way it terminates in the north. It is somewhat open-ended to the extent of how far it continues to the south.

The fact that we do not have a good map of the thickness of the quarternary segments in the New York area prevents us from really saying whether the quaternary is involved in this particular time.

Although we did see warps in the sonar record less than 10 meters sub on line 11, there have been places where they actually have less than 7 meters of quaternary. So in order to ultimately map whether the quaternary is involved in this area will require reflection work with a higher frequency system, somewhere in the 1000 Hz range, to try to map the quaternary deposits.

Well, this is just the overall summary map showing the position of the fault with respect to Long Island and New Jersey.

I think I will take questions at this point.

(Applause)

MR. OKRENT: Are there any questions specifically for Mr. Grow now?

MR. MAXWELL: It looks like a normal fault. Is that the way you interpret it?

MR. GROW: We cannot locate any dips on the fault the way we have it at the present time.

MR. OKRENT: What I would like to do is have an open discussion period where supposedly a few minutes beyond the time when we go into a discussion of the Central U.S. But I would like to have some time for open discussion on the topics already discussed. So why don't we throw the floor open for comments.

Dr. Thompson.

MR. THOMPSON: I would like to go back to the block model that Dr. Hamilton discussed and ask him if he has thought about the significance of the model through time. As I understand the block model, there are relatively coherent elements with the earthquake activity concentrated around the edges of them. And one thing has the accumulation of this activity or movement through time, it would seem to me that it would require there the interconnections and long changes over a long period of time or you simply could not get those concentrations.

MR. HAMILTON: I think we have to draw a distinction between the mid-continent and the Pacific margin. The geologists I talk to tell me about up-down faults in this area, particularly in the Rome trough areas and others, where they see faults that move in one sense or periods of time and then move in another sense. And I think that is an important observation.

And I think another observation which is important is the continuity which exists in certain gravity and magnetic features in the mid-continent area. And these were features that were probably formed in the late Pre-Cambrian or very early Paleozoic. So to me that means that although blocks may exist in mid-continent, they have not moved very far in one direction.

This is in contrast to along the southern margin and the eastern margin of the continent where we know that our model calls for the opening and closing of oceans. So although there may be some situations for organized long-term movement among these blocks in the mid-continent and certainly you can see some trends that go virtually for hundreds of kilometers, I do not think you necessarily have to look for long continuous features with large-scale offsets.

Is that what you were after?

MR. THOMPSON: I think so, Bob. But I wonder. Take the New Madrid area, for example. If activity were simply to concentrate there for a long time, for a geologically long time, what would happen?

Of course, if you think it is like up-and-down motion, locally, one can imagine that. But if it is in response to a horizontal strain, then I would not think it would be possible for it to localize there unless it were at least interconnected with a seismic deformation extending much farther.

MR. HAMILTON: I think you must think of these features in terms of a changing stress field. For example, in the New Madrid area, when the rift originally formed, presumably it was under an extensional regime that was oriented northwest-southeast, the current.

And then later on when we had a continental collision, presumably there was some kind of compression from the generally southerly direction. So there was another stress regime it was under.

And then when the Atlantic opened, we had a different situation. The current stress field appears to be east-northeast depression. And at the present time we see a feature that was originally a rift moving under predominantly right lateral strike slip motion. So this feature is a weak place in the crust that has been influenced by different stress fields.

MR. THOMPSON: I think you are thinking a bit longer term than I am. If we are to understand the earthquake hazard in whatever length of time we have to look and maybe try to look at a 100,000-year window in the New Madrid area, do you think it can remain as a localized feature there, or do you think in response to this east-northeast stress, must things happen over a longer belt?

MR. HAMILTON: I do not really have a very good answer for that. The basic problem I see both in the New Madrid and Charleston area is that in both of these areas we have a precretaceous unconformity, and in both New Madrid and Charleston that unconformity is not very much deformed in New Madrid. Even across the main seismic zone, the deformation ios only on the order of several tens of meters.

Maybe if you summed the deformation across the small fault, you are only talking about a few hundred meters of deformation on that surface. Now, how can this be if this is a major zone of deformation?

VOICE: You are talking apparent dip slip, not the strike slip you can see.

MR. HAMILTON: I am talking about vertical deformation. And I was just going to say in the New Madrid area perhaps you can appeal to a predominantly strike slip motion. But if you have really got a strike slip motion going on for a long period of time, and you have large-scale horizontal deformation, I would suspect there would be a substantial disruption of the overlying sediments. It would require pure strike slip motion almost.

And also, you do not see the kind of offsets like at the head of the embayment or in major features that cross-cut that area. For example, the magnetic anomaly that cuts across at the head of the embayment, that is more or less subparallel with the St. Genevieve fault zone.

That anomaly is pretty much continuous through the head of the embayment down to Tennessee. If there has been large-scale strike slip movement, I would have expected to see some disruption of it.

I think the only way you can account for this is that motion is episodic and that these blocks have to be viewed as jostling one another but never really completely separated very far or having the movement continue for a long period of time geologically speaking.

This is just a nice idea I am advancing. I am not as certain of it as I may sound. But I think there is a real dilemma in trying to account for the lack of disruption in these areas, and I think you have to appeal to some sort of a model along those lines to explain it.

MR. THOMPSON: But isn't the consequence of that model that if you accept it as you just described it, that the motion must shift sporadically to other locations than this?

MR. HAMILTON: I think that is right, that is right. And I think as the stress field changes through time, a longer time than you are talking about, I think you would expect that different fault systems would become active.

MR. THOMPSON: Thank you.

MR. OKRENT: I would like not to focus deeply on the New Madrid area, since we are going to talk about it at a later date.

MR. SEEBER: I would like to address my question to Bob Hamilton. It is about the New Madrid area. My name is Leonardo Seeber, from Lamont-Doherty.

I would like to ask Bob Hamilton, in light of the comment that the precretaceous unconformity is not very much disturbed in the New Madrid area, do you think that the seismic plane or features that have been defined describe all the active faults in the area?

And if not, like the discussion earlier today seemed to indicate, do you think that the locations assigned to the greater earthquakes in 1812 and 1811 are correct or alternatives may be possible?

MR. HAMILTON: The seismic coverage that we had in the New Madrid area was not, of course, as extensive as we would like. We had about 200 miles of coverage. There could be major features in there that we could not define.

There is also a fairly major oil play going on in that area, and about 5000 miles of seismic data that have been acquired. We have seen a very minor part of that data. We know the feature we found associated with the main trend is confirmed in some of that data, although the zone of disruption might diverge from the main side and return toward the north.

So I guess what I am saying there is there may be other features, but I am not aware of any evidence at the present time that there are.

As to whether the 1811 and 1812 earthquakes occurred on that trend, I can only go with the locations Otto Nuttli has derived based primarily on intensity data and the zones of disruption that we found.

I think the evidence is consistent with the locations. The geologic evidence we have derived is consistent with the locations Otto Nuttli derived based on the intensity data. It seems to me the story hangs together, although you cannot say for sure there is not another structure and that the locations could not have been somewhere else.

MR. OKRENT: Are there any general questions for the New England area?

MR. BAROSH: I wanted to make a comment or follow up -

MR. OKRENT: Your name is?

MR. BAROSH: Pat Barosh, Western Observatory.

I wanted to follow up on what Bob Hamilton was mentioning about the faults. We see that same sort of thing through New England. And as mapping has progressed particularly over the last ten years, a great, long early Paleozoic fault zones are developing. The fault pattern is going to look like California with very long faults eventually. Walt Anderson discussed one. There are a number of others.

And on these we can see that there have been great numbers of times they have been reactivated from very deep ductile faults to more brittle recent sort of surface activity. It can be in different directions. And these are the faults that come out, start popping out in the mapping.

But actually, what we curiously have been discovering of late the last few years is that these long faults are cut by other tiny faults that are much younger and these little faults, which appear to be younger and perhaps more pertinent to the seismicity in the region, tend to have small displacements.

It is only in quite detailed mapping that you do see they cut and offset t ' old ones and the old ones have not been reactivated to reactivate them. But it is difficult to get a handle on their age.

MR. CHINNERY: May I take two minutes to explain all of this stuff?

MR. OKRENT: You can try.

MR. CHINNERY: I think the problem in the Eastern U.S. is to try to make too close a connection between faults and earthquakes. I think each time we come across it, we have the same trouble. We seem to have an overall stress which is large scale, presumably related to plate motion. It is hard to believe in my mind that it has not existed for some tens of millions of years, perhaps longer.

Had we had a history of seismicity throughout that period, we would expect to see evidence of recent motion on the faults if it really is reactivated in some way.

As I understand it, there is no indication that there is any recent motion in these ancient faults we see. I do not know if you see it that way, but this, to me, is the big dilemma.

I think the other way of looking at this -- and I am a great believer in the concept of stress concentrations because I think that is what will happen rather than reactivation of these faults -- I think what we have instead are a whole series of different structures in the East which are concentrating this overall plate stress.

I think there are many kinds of structures capable of doing this. I think platonic intrusions are one. I think a logical candidate for stress concentrations are faults, because one big characteristic of faults is that they juxtapose different rock types on either side of the fault. And I think it is exactly this kind of structure which will concentrate stress.

I think structures which are particularly affected is where you have intersect faults because there you get even more stress concentration.

The key point in all of this is, if there is anything in it at all, is its tremendously shape-dependent. The nature or the extent of the stress concentration will be very dependent upon the shape. Smooth, round structures will not do much to you. But you get little pointed structures, offsets on faults, for example, little pointed things will give you very large stress concentrations, giving you the right combinations.

I throw this out only because it is te way I listen to all of this kind of evidence, and I think it is a little bit different than thinking of it in terms of actual reactivation of faults. Faults are merely trends along which you find major stress concentrations. And I see incredibly complex structures in some of these areas -- Grabens, for example -- and I say, great, this is just what I need to produce rhese kinds of boundaries between rock tribes which can give rise, I think, to the stress concentrations. We see these then in terms of earthquakes, and we do not necessarily expect to see actual recent motin on the faults involved. That is my theory.

MR. OKRENT: Mr. Page.

MR. PAGE: I think the very fact that we do not see large scarfs in the eastern part of the United States and other surface indications of faulting means that either displacements are not very large or they are very infrequent in any one place.

And I think that both Bob Hamilton and George Thompson implied that we should not be totally focusing on places of presentday seismicity, because we see that the faults that are associated with earthquakes do not have tremendous surface expression, probably have not moved great amounts frequently -- during geologic time, that is, say, during the last few million years.

And this makes me wonder whether the activity at New Madrid and Charleston are just aberrations of the prsent day and age, something that is very transitory, and whether that would mean lasting several millenia or maybe a million years, I wouldn't know.

But it is pretty obvious to me that activity will appear somewhere else sometime instead of those places, because we do have the faults and we do have the whole continental plate under stress and we do not have geologic signs of recurrent great activity with great frequency in any one particular place.

I am often reminded of the fault movement in Western Australia. I cannot remember the name of it. Was it Lizburn or something like that?

MR. HAMILTON: Makiringem.

MR. PAGE: Yes. Which involved a surface displacement. It was rather unusual for the interior of a plate, but as I understand it from the literature, there was no topographic indication that things like that had happened before, say, within the last thousands of years or few millions of years.

This would seem to say that you can have a fairly important event at very great intervals.

VOICE: I would like to point out that Ben's comments are very well supported by the seismic reflection data at New Madrid -- if I can still refer to New Madrid for a few more moments -- and that the episodic nature of the motion is very striking, that the geomorphology shows that there have been numerous earthquakes in the past few hundred years.

And yet the geologic record shows there is no way you can extrapolate that rate of activity even for several thousand years. You would see much larger offsets than you do.

So, clearly, the motion is episodic in time, and I think one explanation of that that is very logical is that the origin of the stress field, which is regionally quite uniform, is plate motion, which is a very gradual procedure, so that after you have a series of earthquakes at a given locale and you have reduced the stress due to those earthquakes, it may take thousands and thousands of years, perhaps hundreds of thousands of years, to build the stress back up to the failure level.

So I think these arguments all sort of tie in well together. You would expect a recurrence time before bursts of activity at interplate areas to be very long. And in fact, the only evidence we have which is at New Madrid says that they are very long.

I would like to say, however, that you do not see any evidence -- and the data is not very good -- we do not see any evidence of localized changes in the stress field near faults at places where we have independent measurements of the stress fields, say, due to in the western U.S. the young volcanic beaters where you have fault offsets. We have earthquake focal measurements. We have in situ stress measurements.

They all show the same direction and relative magnitudes of stress fields, which seem to indicate that local magnification in all directions of the stress field is not an important factor.

Of course, the data in the eastern U.S. is probably not sufficient to make the same argument.

MR. OKRENT: I wonder if I could pose a question to Mr. Jackson's staff. I thought I heard a comment to the effect that the New Brunswick earthquake might equally well occur in Maine or so forth. Do you have anything to say about that? Does it have any implications?

MR. JACKSON: I do not think I said that.

MR. OKRENT: No, no, you did not.

MR. JACKSON: It has been in the press, and I do not know the answer. I think there is some question in my mind whether that earthquake is in the Piedmont geology. We have not looked into it, but I am sure the people here can make a lot better comments on that than I.

If it were, the design basis earthquake for plants in the Piedmont region, around the magnitude of 5 to 3, taken as a design basis earthquake, that this would be larger than that. But I do not know the geologic relationship to structure or how it relates.

Whether that is indeed a Piedmont, a New England-type Piedmont earthquake, maybe Mr. Ebel, Mr. Barosh or others could comment on its relationship to regional tectonics.

MR. OKRENT: Do we have a volunteer?

VOICE: First of all, as far as the location is concerned, the best location that I have seen to this points puts it sort of in the middle of the tectonic body in central New Brunswick, and I am not that familiar with the tectonics of that particular area.

In terms of the seismicity and what is known of that area, of course, the historic record is very incomplete because the area has been and is today very sparsely settled, although we do have some epicenters from the area. And in fact, the evidence I have shown of the foreshock activity would suggest that there was some prior seismicity leading up to the event of January.

As far as the question of whether or not the event could occur in other areas, the one thing that I have noticed, the event seems to have been felt down to New York City and that seems to have been about the limit of the felt area, which would suggest that in terms of felt area it was roughly the size of the 1755 event, just looking at a distance measurement.

So on that basis, I would have to say that if the 1755 event of Cape Ann could reoccur, it would probably look something like the New Brunswick earthquake. Insofar as the two larger earthquakes that have occurred in January, both have been in areas where we have recorded previous and very recent seismic activity.

MR. OKRENT: I think you are answering a different question. Maybe you do not want to answer the question I posed, but someone during this morning I think said that the New Brunswick earthquake might equally have occurred in Maine, unless I misheard them.

VOICE: Well, what I was going to say, to continue on --

(Laughter)

-- was simply, since we have had the seismicity prior to both the New Hampshire and the New Brunswick earthquakes, we are recording earthquakes in Maine. In fact, since the January 9 event we have had one or two small earthquakes occur in Maine. So I do not see why we could not have an event of that size there.

MR. HERRMANN: Bob Herrmann, from St. Louis.

I think the important thing about the New Brunswick earthquake is in the next year you are going to have lots of seismologists studying that earthquake who will get the focal mechanisms. There will be two planees. Perhaps one will be parallel to some geological structure. That would be nice.

(Laughter)

MR. OKRENT: If there are other comments on the specific question, I would appreciate it.

MR. SYKES: I was going to try to address this topic this evening. Len Sykes, Columbia University.

But let me just advance one point of view here. My sense, in following the work that has gone on in New Madrid, Charleston, and other places in the eastern U.S., is that New Madrid has certainly had the largest earthquakes. It also has had the most frequent number of magnitude 5 or 6 earthquakes. It has the clearest evidence of deformation throughout the Zenoic and back into the cretaceous.

So I would tend to see it as our most unique and clearest source with a geological expression in the central and eastern part of the country. And also with the repeat times appear, the ones of 700 years or so, while they are long in comparison to the San Andreas, I would see this as still quite frequent perhaps as compared to New Madrid or other sources along the eastern seaboard.

And I think then that you have to entertain the possibility that an earthquake like Charleston of a magnitude of about 6.75 could in fact occur on a number of faults along the eastern margin of the U.S. We need not have a fault 100 kilometers long in order to produce a Charleston-type earthquake.

But the other corollary of that is that we know an earthquake like Charleston only occurs roughly every once or few hundred years along the eastern margin. So if other faults are about as capable as that, then the frequency of occurrence must be something like 100,000 years or a million years for a Charleston-type earthquake on any one particular fault. Then the magnitude 5 earthquakes being roughly 10000 to 10,000 years.

MR. BEHRENDT: John Behrendt, USGS.

Along the lines of what Len Sykes just said, I would point out that I will be showing some reflection profiles this afternoon, but these very small, probably reactivated Triassic faults that we see in the Charleston area indicate that maybe at most 50 meters of vertical displacement in 100 million years, which would suggest a very long recurrence interval, if one is going to associate the Charleston earthquake with such a thing.

MR. OKRENT: Well, does someone want to discuss specifically the New Brunswick earthquake and where else it might occur?

MR. BAROSH: I think I am the guilty one. I mentioned that during my talk.

MR. OKRENT: I knew I did not dream it.

MR. BAROSH: But it is essentially what John Ebel mentioned. The cluster of activity that we had prior knowledge of in New Brunswick is not that greatly different from the cluster of activity that extends north of the Penobscot Bay in Maine and the one that goes inland from Casco Bay in Maine, the same kind of history.

And if we had judged those areas prior to the earthquake, we would have probably roughly equated the three regions, at least observation would have. And having an earthquake that size in New Brunswick, which actually in terms of intensity there have occurred before there, but knowing the wide spread, we would have no reason to say that one could not occur in northern Penobscot or north of Casco Bay.

MR. OKRENT: Mr. Philbrick, did you want to comment?

MR. PHILBRICK: He is talking about northward-striking faults. Isn't that right?

MR. BAROSH: I was just talking about locations. There are clusters of earthquake activity, and they are on north and northwest trends in those areas.

MR. PHILBRICK: If you want to project that thing from New Brunswick into Maine, you have to project it southwest.

MR. BAROSH: I was not implying there was any direction. I did not mean that. I was just talking in terms of activity. We have no reason to connect the two by fault zones.

MR. PHILBRICK: All right.

MR. OKRENT: All right. Thank you. We may come back through all of these areas as we go on, but I think we now had best begin our formal discussion of the central U.S. New Madrid area. Mr. Buschback.

MR. BUSCHBACK: The New Madrid study groups consist of more than 30 highly qualified scientists engaged in studies, geology, geophysics, and seismology, in the New Madrid area. The study is funded, in part, by the NRC.

It is aimed at determining the earthquake risks in a 200-mile circle around New Madrid. This is to define the structural setting, tectonic history, and ultimately to determine the relationship of current seismicity.

The capital letters on this slide show the organizations which have been involved in the last year or two with funded research through NRC. On the lower left you will see the USGS, TVA, and the Army Corps of Engineers. They operate basically on their own funds, but they exchange information. They give us their programs. We give them ours. They meet with us on our progress meetings, and they have contributed some very critical information.

Altogether it has been a team that has been able to react and respond to questions from the NRC. And it has developed a nice relationship between states and the NRC.

The area is interesting geologically. The Ozarks on the west side, in pink, bring Cambrian ordovician rocks to the surface. Some Pre-Cambrian. The grays at the north are the Illinois basin. The pinks are ordovician rocks, coming onto the Cincinnati arch, extending into the Lexington dome and the Nashville dome.

The yellows, oranges, and greens are tertiary cretaceous rocks coming from the south, outlining the Mississippi embayment.

Some of the important structures that do not appear on the geologic map are the Pascola arch and the Reel Foot basin in the northern part of the Mississppi embayment, and the Rome trough in northern Kentucky.

The New Madrid area being seismically active, the NRC and the USGS, under separate programs, have funded seismic arrays. Memphis in the south, New Madrid in the center, and the Wabash Valley array going up to the northeast.

The Wabash Valley array was set up because one of big questions in our entire study has been potential northward extension of the New Madrid seismic zone. Dr. Herrmann will certainly discuss the seismicity. I will not even mention it.

Now, I do have to show a two-year plot showing concentration of the earthquakes around the New Madrid area and note the dog legs. Earthquakes are associated with active faults, or should be. You are releasing stress deep within the rocks. They should be associated with active faults.

We have many significant faults mapped at the surface or near surface of the bedrock throughout the area. And virtually none of these show any activity, any recent movement, and are not associated with current seismic activity. There must be active faulting associated with those earthquakes at depths of a couple of kilometers down to maybe 20 kilometers, the hypocentral depths of the earthquake. And, of course, there must be an association with the tectonics of the area.

Geophysics offers us one solution. This is a Bouguer gravity map. Our group has cooperated and funded several of the gravity and magnetic studies. The Purdue group put these together. This is just the west half of the sheet, representing about 50,000 readings.

The data have been digitized. They are gridded on a 2-kilometer grid, as is the magnetic map. It is based on 28 surveys, and integrated 28 surveys. This too is gridded on a 2-kilometer grid available on computer tape.

These maps were recently published at a scale of 1-to-1 million. I will not call attention to the geophysical features, because that too will be covered.

I do not have time for very many of the programs, but let me give you a few of the state programs.

The Illinois survey, at our urging, has made detailed maps of the Wabash Valley fault system. In concert with that, the Indiana survey studied the faults, the Wabash Valley faults in their state. We now have very detailed good maps in both states, a very critical area in our study.

No state line on conformities. The maps do fit.

Fundamentally, the Wabash Valley faults have been fund to be hig-angled normal faults, Horst and Graben arrangement. They are dated as post-Pennsylvanian and pre-Pleistocene in age.

Much of the mapping and the work that the states do is mapping in and near suspected or known faulting. This is the upper part of the Mississippi embayment, the cretaceous and tertiary rocks covering the southern tip of Illinois.

The faulting that is shown in the Paleozoic rocks ends abruptly at the cretaceous cover, not because they stop but because they apparently do not break the cretaceous cover. Certainly, they project southward under there.

When we strip the cretaceous off, the mapping of the Paleozoic rocks under there is important to us simply because we need to know the lithology in southern Illinois. We have a post-creek cutoff, an obvious fault with Pleistocene mounds gravel on the left against the McNary sand.

However, in drilling, our ultimate determination was that this was a solution collapsed feature as opposed to a tectonic adjustment. Everywhere in the embayment were carbonates; limestones are overlain by the cretaceous sands. Solution is a possibility.

In our subsurface work, we certainly inventoried all available deep wells. You notice the relatively few Pre-Cambrian points available in our entire area. Howie Schwab prepared a map. Basically, the top of Everton Knox is a Cambrian ordivician carbonate. It is one relatively deep.

I am showing you only the Cambrian and ordivician part of our studies. The lower parts, sea level to 1000 in the yellows; 5-6000 feet in the Illinois basin; below sea level in the orange; and the blues, the Knox, the purple in particular -- Knox has been cut off entirely, just been removed by erosion -- but a brief disruption would put that about 8000 feet above sea level. The orange at the south is about 10,000 feet below sea level, just over 10,000.

Taking that datum and starting to map downward now, thickness of the Knox shows this Reel Foot basin 7000 feet of carbonates that were collected in the general area of the so-called Reel Foot rift.

Notice also a certain westward thickening toward Kentucky, a little east-west trend there.

The thickness of the pre-Knox, which is everything from this carbonate, thick carbonate wedge, down to the basement rocks, include mostly Cambrian clastics, certainly, early Cambrian.

Another wedge or thick trough of sediments going up the Mississippi River and very definitely an east-west in-filling of thick sediments, old sediments.

A couple of cross-sections, north-south through Illinois, shows the Rough Creek Graben, fault near the Cottage Grove in Illinois, so-called, with a relatively sharp drop to the south, and including some of the oldest sediments that we know of in Illinois, indefinite dropoff and Graben filling.

East-west in the area you see thinning on the west toward the Ozarks, thinning on the east toward the Nashville dome, thickening through the Rough Creek Graben area. And then almost all of the Paleozoic. This was not particularly a one-shot deal.

The north-south cross-section farther east is hung on the top of the Knox, and it the vertical exaggeration is a little startling, but it does show the Rough Creek Graben without any question. We do have some middle-Cambrian dates from trilobites in there, so we know it is older than the rocks that we thought we had in the region.

One of my pet projects is mapping the Pre-Cambrian, keeping a current map. Pat Barosh put some color on one of my recent maps, and so I will mention briefly. You can see the east-west faulting in the middle of the green there.

And I would guess that is about 10,000 feet, Pat?

MR. BAROSH: Yes.

MR. BUSCHBACK: The blue is getting around 20,000 feet. You will notice the thick, the low Pre-Cambrian in the Graben, and this funny-looking nose sticking out from the embayment is the Pascola arch. The Pascola arch is an enigma in our area. It rolls out of nowhere about the end of the Paleozoic time, about 10-15,000 feet of sediments were enplaned off the top of it; that is, if you rode it to a base level covered by cretaceous sediments.

And as far as I can see, there has been no tendency whatsoever for differential uplift in that area since then. It is probably bounded by faults, and it does have a very interesting relationshiop with our current seismicity.

In summary, these are what I see as the features that existed near the end of the Pre-Cambrian time. The Ozark dome and the Nashville dome literally have been relatively positive, although they may have taken lots of sediment down. But in relation to the area around, they have been relatively positive ever since then.

And while I have the slide up, let me talk about reactivation. Now, Rough Creek Graben is a positive -- it is not positively identified. There is not any question that it exists. It is downdropped to the south. At the end of the Paleozoic time in the Rough Creek system it is downdropped to the north.

This is beginning the Paleozoic. We dropped it down to the south. At the end of the Paleozoic we dropped it down to the north. Both along pretty much the same line, as far as we can see.

The Reel Foot rift, of course, again an early Paleozoic, late Pre-Cambrian feature, and that certainly was reactivated during cretaceous time as with the downdropping to receive the embayment sediments.

This is Howie Schwab's attempt at a three-dimensional view of this same area. Toward the end of the Paleozoic time, before the Pascola arch, none of us can really visualize what that Pascola arch did to this picture of the Graben being downdropped and coming out.

But anyway, this is just before the Pascola came. And you can see what Howie calls the Mormon deep south of the Rough Creek fault is our Rough Creek Graben. Reel Foot rift has a kind of an interesting portrayal there, and I assume that it is right, because the sharp boundaries and downdropping the entire section may not literally be. I would guess that there is just not a steady downstep, certainly a considerable amount of breaking and faulting through there.

MR. OKRENT: Mr. Buschback, we will have to finish in a minute or two.

MR. BUSCHBACK: All right, sir.

Tom Hildenbrand, second order, second vertical derivative is certainly the most startling portrayal of the presence of a rift through our area. Tom shows the area, the rift bounded by interpretive plutons, mafic, ultra mafic.

Certainly, our study has contributed -- and this is Bill Hinze's slide, and I apologize to him -- contributed to the development of models in the New Madrid area. And, hopefully, Bill also will speak on the potential extensions of a geophysical linear geophysical features that seem to identify this rift down in the New Madrid area.

As has been mentioned several times today and in the last talk by Dr. Bushbach, the main feature we have been able to identify as relating to measures of seismicity in the New Madrid area is the real foot rift shown here outlined in white solid lines. The orange, of course, is the outline of the embayment for reference. The rift was identified primarily on the basis of gravity and magnetic data, and also by seismic retraction data, and to some extent seismic reflection data, as pointed out by other speakers.

It is bounded along the margins by mafic or ultra-mafic plutons shown here in light green, by far the majority of them occurring along the northwest margin as compared to the southeast margin. We feel this may be due to the fact that the northwest margin has undergone somewhat greater disruption than the southeastern margin, where it may have in fact acted as more of a hinge zone during periods of subsidance.

I might point out that some of the previous talks have pointed out the rift is a steeply dropping Grabens structure, but in fact the Grabens have dips on the order of 12 degrees, so the drops are rather subtle. It has a structural relief of one and a half to two kilometers between the center and outer sides, and it is about 75 kilometers wide.

Looking at the relationship of the rift to the seismic patterns, this is a portion of the area in the Boot Hill of Missouri and northwestern Tennessee. The rift is shown and outlined by the solid white lines running to the northeast, and the plutons are shown as lavender colored bodies. The white dots are the locations of epicenters of primarily micro-earthquakes as reported by the St. Louis University network over the last several years.

As you can see, as others have mentioned, they generally tend to occur in distinct linear patterns, one of which runs down the axis of the rift. From near Marked Tree, Arkansas, to Carruthersville, Missouri, there is another general linear northeast trending pattern which doesn't show as well in this slide as on other plots. It extends from just southwest of New Madrid up along the margin of the rift towards Charleston, Missouri, which as Bob Hamilton points out had a major earthquake in 1885. We had a major cross-zone of earthquakes, a very diffuse zone of abundant activity from near Dyersburg, Tennessee, to just west of New Madrid, so I might point out the earthquakes in this cross-trending zone are more abundant than those in the northeast trending zones.

However, the larger events tend to occur in the northeast trending zones, and as a result, the amount of strain energy which has been released in the last several years is perhaps four times greater on the zones along the axis here than on the cross-zone, even though the earthquakes are more abundant there.

In an effort to try and learn more about the relationship between the,zones of seismicity and the rift, the U. S. Geological Survey did run a couple hundred kilometers of very precise seismic reflection profiles several years ago, and the locations of these lines are shown in yellow. They are primarily along and across zones of active seismicity. The results of that survey show several very interesting facts with regard to the distribution of faults in the area.

We were first of all surprised we did not find more faults than we did. There does not appear, at least from the amount of surveying we have done, to be as abundant an amount of faults as had been identified in rifts in other parts of the world. The faults which we do find tend to concentrate along the axis of the rift, such as these faults here in northwestern Tennessee and this major zone here, and along the margins of the rift just adjacent to New Madrid here.

We also tend to have a number of cross-faults which trend to the northwest and are not clearly shown in this particular slide, but which I want to mention, which break the rift into a series of blocks. We have had mentioned by Bob Hamilton and others the possibility of block tectonics in this area, and just the last talk that Tom Bushbach gave with the Pascola Arch, the mention of blocks.

We seem to feel that in fact the rift is broken into a number of blocks by northwest trending faults, and in fact I feel quite strongly that the Pascola Arch itself is the expression of one or more of these blocks. The dips we do see on seismic reflection profiles do not support the interpretation of the Pasocal Arch as a classical anticlinal dipping fold with dips away from a central anticlinal axis. Rather, it appears to be sort of a block feature.

The faults which you do find in this area generally have very moderate amounts of displacement, with the exception of this fault zone in northeastern Arkansas, where we have interpreted perhaps as much as one kilometer vertical offset in the Paleozoics. We actually have very little amount of displacement, perhaps only as much as 80 meters at the extreme on post-Cretaceous Age faults. This would be indicative of the Cottonwood Crow fault occurring in northwest Tennessee at this location here (indicating).

Generally, the Paleozoic surface is not very disruptive. It does not show large amounts of structural relief.

One interesting facet about the faults which has been mentioned already is the nature of reversal of movement on some of these faults. This reversal can be clearly seen on a number of precise reflection profiles. There is an effort of extensional moving faulting in early Paleozoic, compressional moving in late Paleozoic, and again extensional moving in late Mesozoic time contemporaneous with the formation of the Mississippi embayment, and then depressional reverse fault movement in the late Tertiary and into the recent.

I might point out, too, that the nature of the Paleozoic bedrock surface appears to be affected and associated with zones of linear earthquake patterns. Where we have concentrations of linear earthquakes crossing reflection profiles such as here, here, up in through here, we tend to find a Paleozoic bedrock surface which has been highly broken up and fractured, yielding a scalloped like appearance. In areas where there are not major zones of earthquake concentrations, this particular Paleozoic bedrock surface is relatively unbroken.

Looking now at a picture of the Cottonwood

Crow fault, just to give you an example of what a fault looks like in the area on a reflection profile, this is in northwestern Tennessee. The blue line here is a Paleozoic reflector, the red line a Cretaceous reflector, reflectors above this in the Tertiary section. This particular fault underwent perhaps five to ten meters of normal displacement during late Mesozoic time, followed by approximately 65 meters of reverse displacement in post-middle Easean time.

The history of the guaternary deformation is generally one of uplift and faulting. Perhaps the most striking deformational feature in the area is what we refer to as the Lake County Uplift, which is shown by this map as being a zero contour of uplift, which has brought the Mississippi River flood plane upward as much as 30 feet in the last 3,000 years.

Here is New Madrid, for reference, in our location. This particular uplift appears to have occurred during earthquakes. It is not apparently an aseismic feature. Most of the uplift which produced it apparently occurred during earthquakes. Along the eastern margin, which is very steeply dipping as compared to the rest of this uplift feature, we have what is known as the Reelfoot Scarp, and a trench put by the USGS along the northern margin of this scarp several years ago detected several faults, and liquefaction features in the trench would suggest that in this area we have had three large earthquakes in the last 2,000 years which yield generally an average recurrence interval of about 600 to 700 years per major earthquake in this area, and this would be earthquakes large enough to cause the surface faulting and liquefaction.

I might point out this is the only geologically determined incident for recurrence intervals in the New Madrid area.

I have taken the picture at the uplift here, and have tried to develop a model to explain its occurrence, and why it is where it is located today, and this particular model is shown on this diagram on the screen now. There is a right lateral strike slip fault, is our interpretation from Bob Hermann's fault plane solution data of these northeastern trending zones of seismicity from Marked Tree, Arkansas, up to Carruthersville.

Likewise from Bob Hermann's work we have the northeastern strike fault from west of New Madrid up to Charleston, Missouri, and lying between these two segments we have a margin in the area of the Lake County Uplift, and as a result, I think, from the general fault movement that we would have from these two segments, we would find that we have compression due to the relative block movement in the area between these two offset segments.

Therefore, I interpret the uplift as being a result of vertical strain due to this compressional motion on the two major northeast trending segments. Analysis of this sort of alignment, where we have what we call left stepping, as you view it from the southwest, left stepping, right lateral offset, we believe may be important in terms of the amount of strain release which could be associated with future earthquakes. This sort of left stepping pattern has the capability of storing a vast amount of strained energy which may be released in large earthquakes in the future.

We tend to do more work on the nature of fault interaction of segmented faults. I would like to take some time now to discuss some of the more recent results we have obtained in our work. From the gravity maps which Bob Hamilton discussed this morning which are outside in the hall here, I have developed this, at least from parts of his maps, this particular slide, and there are a number of very interesting features, particularly on the 125 kilometer high pass wave length filtered map out there, which has not been detected in other maps or other work before.

One such feature or two such features are northwest trending gravity loads, very prominent features cutting through the mid-continental area and the Mississippi embayment. First, the one in red I would like to discuss, and then briefly this one outlined in dashed white lines.

This red feature here can actually be traced as a very distinctly linear feature all the way from the Snake River plane down to where it is lost against the Appalachian front. So it is a transcontinental feature of major proportions. Where it crosses the central North American system, we have a major offset in that system which has been generally suggested as being a transformed fault. It cuts across central Missouri, along and parallel with what is known as the central Missouri basement high, and in general an examination of rock maps indicates that rock within this particular gravity low are somewhat older and more metamorphous than the rock adjacent to it.

Examination of structural geologic maps show there are numbers of faults throughout the geologic section which parallel and lie within this feature. So we interpret this gravity low as a major zone of deep-seated structural weakness which has persisted for many millions of years and is clearly older than the central North American rift system and the Reelfoot rift system.

Of importance is the relationship which occurs where this gravity low crosses the Mississippi embayment. It crosses the embayment precisely where the seismicity takes its jog from a northeast trend along the axis to the north-northwesterly direction. Likewise, this is the exact location of the Lake County Uplift and the location of the Pascola Arch. There may be some relationship between this long-standing zone of crustal weakness and the formation of the Pascola Arch in the Paleozoic time.

A suggestion which should be studied in more detail then would be that one possible reason why we have such a concentration of earthquakes at this point in the Mississippi embayment is that we have the intersection of two major zones of structural upper crustal weakness, this northwest trending gravity low and the Reelfoot rift.

Looking briefly at the northwest trending zone southwest of the first, this particular feature is best displayed structurally in southwest Missouri, where there is quite a bit of drill data and where it is seen to be a depressed basin, perhaps a large major Grabens with numerous faults also within it. Faults can also be seen parallel within this feature, just outside of the embayment on the opposite flank of the Arcoma basin and also faults in southwestern Tennessee just west of Memphis.

It could possibly be the relation of this particular gravity low here and the seismicity in the Reelfoot rift area, because this particular gravity low cuts the Reelfoot rift near Marked Tree, Arkansas, exactly where the seismicity tends to die out, and it might be suggested that the seismicity stops at this point because a fault or some other structure within this gravity low may act as an asperity to a propagating or rupturing fault within the rift itself.

This particular suggestion or hypothesis must be studied in greater detail in the future to try to better pin it down, but in fact, as Bob Hamilton pointed out, the rift does continue past this particular gravity low further to the southwest and Arkansas. This is a Bouger gravity map of Arkansas. Bob showed the magnetic slide. The existence of the buried Ouachta relational structure front -- this is a complement in the gravity down to approximately this location here. This is a magna cove pluton and other plutons southeast of the magna cove. But the rift would continue down to approximately this major gravity radiant here, and we could say that no doubt the major New Madrid style earthquake would terminate at this point here as its possible southwesterly most extent.

Bob also mentioned a major refraction program which is a survey worked on in the last couple of years. I would just like to add one point to that, and that is the interpretation of this particular line, refraction seismic line down the axis of the Reelfoot rift. This work has been again done by Bill Ludder of now Purdue and others in the Menlo Park office of the Survey, and this is their interpretation of the steep structural model, and you must keep in mind this is not a cross-section across the rift. Rather, a longitudinal profile down the center.

I would like to point out their interpretation of a low velocity zone lying beneath the rift and down into the upper crust into the basement of the upper Mississippi embayment area, where they have a zone of 5.6 kilometers a second interpreted to be sandstone. We believe we have evidence for the existence of this particular -- feature in one of our seismic reflection profiles.

This is one I think Bob Hamilton showed earlier. It is an east-west profile now across the rift. This particular reflector we interpret to be the top of the magnetic basement, at about 4 and a half kilometers depth. This particular reflector here, which is generally horizontal, existed at about 8 kilometers depth, so this wedgelike interval which thickens toward the axis of the rift, the axis being here (indicating) we believe could possibly represent the zone of low velocity sediments identified on the reflection profile, and by the way, this particular dipping reflector at the top of the basin is the same northwest dipping reflector Bob Hamilton was discussing in reference to the Pascola Arch a few moments ago.

We have also in the last year been conducting a rather large boat operated seismic reflection experiment on the Mississippi River. The boat has been provided by the Marine Zoology Group at Woods Hole, and funding has been provided by the Nuclear Regulatory Commission. We ran a line from approximately 50 miles north of Memphis all the way to just about the confluence of the Ohio and Mississippi Rivers, so we had well in excess of 200 kilometers of new seismic data which will help us to intertwine our vibroseis lines as well as provide a lot of new data which we have yet to interpret.

We have only processed one half of the data, and I would like to point out two features we see from this one piece of line here from near Carruthersville, Missouri, at the southern tip of the boot hill of Missouri. This particular dotted line is a horizontal datum for reference, and it represents, this wide reflector here represents the top of the Cretaceous bedrock, but you can clearly see a doming in this reflector here. This doming we can trace to the northeast, and we see it on our vibroseis lines in northwest Tennessee. Along the north flank of that doming right here, we can also interpret a piece of the Cottonwood Grove fault which I will show you a slide of later.

The detection of this particular fault on a direct line with the division of the fault as plotted from the vibroseis profiles allows us to say that this particular fault is at least 40 kilometers long in the embayment, and thus may be large enough to support a significant earthquake, particularly in view of Otto Nuttli's recent results on the necessary links for different types of earthquakes in the embayment which I believe you will be discussing tomorrow.

I might point out one other feature of this type of profile, and it is poorly resolved here, but for the first time on a reflection seismic profile we have been able to identify the base of the quaternary alluvium of the Mississippi River. In all of our other profiles of a vibroseis nature we couldn't see above the Eocene, but for the first time we have several hundred kilometers of data to look for evidence of offset of that quaternary contact, and therefore have the chance to detect a possible quaternary age fault from geophysical data as opposed to trenching and other detailed geologic mapping.

I might point out that we have tentatively identified several quaternary faults we have not seen in other manners of mapping. We have also a number of faults deeper in the Tertiary Crutaceous section of fairly large displacements, on the order of 50 meters, which we have not detected by any other method. So we still have quite a bit to do on this interpretation, but we believe that it provides us with a lot of new and important information.

I would like to just briefly mention now several of our just begun projects, and those which are planned for the future. This is a general map of the Mississippi embayment area. We plan to put this summer a new surface trench in near Marked Tree, Arkansas, along a lineant which seems to be parallel with the northeast seismicity, and the purpose of this trench would be to see if we could detect any evidence of a surface fault breaking quaternary sediments which might be associated with this northeast seismic trend and also to see if we might be able to collect some more data which would allow us to determine an additional geologically determined recurrence interval for the major earthquakes in the area.

This fault, too. We have embarked upon a level line program in the area of the Lake County Uplift to see if we might be able to detect whether this particular feature is continuing to be uplifted, as was suggested by a detailed analysis of geomorphic data. We have also embarked on a major program of reprocessing all of our vibroseis reflection profile lines. We have reprocessed some of the lines so far, and we have found a marked improvement due to problems in the earlier processing whereby we now have a 25 to 50 percent improvement in the resolution of a reflector, particularly in our Paleozoic rock section, which is very important, because we are down now in the area of central locations of the earthquakes.

Along with this reprocessing, we might point out that we believe it is very important that the proprietary reflection lines that were shot as a part of the Grand Gulf nuclear site be reprocessed also. We have looked at these lines in detail, and we feel that for a number of reasons, one of which is that they were shot eight years ago and the techniques for processing were nowhere as good as they are now, that the processing is inadequate, and that there are a number of features probably which could be resolved in these lines which you cannot see because of the nature of the processing done.

I would strongly support and suggest that NRC support the reprocessing of these lines to improve the resolution of the structures that they would probably contain. One of our best lines of evidence to show the inadequacy of these lines is that we have several survey USGS lines very close to a number of their lines. Whereas the survey lines show significant structures, the nature of the processing of the Grand Gulf lines show very little.

So, I again would support that reprocessing, and that is it.

(Applause.)

MR. OKRENT: Thank you. Does Mr. Jackson want to comment on your last point?

MR. JACKSON: I would like to offer a comment. It has been a long time since I have discussed it, but my understanding is, we took such a conservative position with the Grand Gulf applicant that there is little merit for them to do it. We have assumed a design basis closer to their plant than these lines are needed to warrant a lesser controlling earthquake of a distance, so it becomes academic, so what you are recommending is the NRC take a hard look at these lines and continue processing them, not that there is any responsibility of the applicant, the utility or applicant, to go back and reprocess those lines. Am I correct?

MR. RUSS: Yes.

MR. OKRENT: Can I ask a related question? You used the boat and went upstream from Memphis.

MR. RUSS: That's right.

MR. OKRENT: Is there any merit in using the boat and going downstream from Memphis to see if there is anything you don't expect?

MR. RUSS: Do you mean work downstream from Memphis to see if we might detect other structures? Yes, in fact, I have talked to several people investigating neotectonics in the lower Mississippi River valley through a contract with the Corps of Engineers out of Vicksburg who are contemplating doing just that. However, I would suggest again going upstream, starting from Vicksburg, perhaps, and working up to Memphis. It is very difficult to work downstream with the current.

MR. OKRENT: I only meant the direction in which the measurements were taken. I wasn't implying the boat had to go in a particular direction.

MR. RUSS: But this may in fact be done, if they follow through with their plans.

MR. OKRENT: This paper is open for comment or discussion in addition to what we have already heard.

MR. RUSS: Tom?

VOICE: Dave, what is the relationship of your southern gravity low south of the major one with two things, one, the Ouachta front and the other the newly publicized or is you top one new?

MR. RUSS: Yes. The top northwest trend gravity low is the one which has received a fair amount of publicity, approximately a month ago, in the newspapers.

VOICE: Do they bring it up that far in the north?

MR. RUSS: They bring it even further to the north into Washington, but that would coincide with the one I showed in red on my slide. The one to the southwest of that still lies northwest of the Ouachta.

VOICE: Thank you.

MR. BAROSH: Dave, you mentioned the dip on top of the Reelfoot rift as being around 12 degrees. Was this on the top or was this from faults that bounded?

MR. RUSS: These are the bounding flanks of the rift, as you move from the high side to the outside of the rift, down to the bottom of the rift. It is not a steep-sided Grabens type feature. The flanks slope gently toward the center at about 10 to 12 degrees. This information is from magnetic modeling by Tom Hilinbrand.

MR. WENTWORTH: Carl Wentworth, USGS.

Dave, do you mean that the bonding faults have that dip, or do you interpret that to mean that the flanks have been eroded to the present configuration?

MR. RUSS: The flanks have been eroded. I guess. I don't mean the dips on the faults. Undoubtedly we have a great number of flanking faults along the margin, and they would be a steeper dip.

MR. RYDER: Leon Ryder, NRC.

I noticed that the gravity lows you plotted there pass through the intersection of the Vinconin gravity anomaly and the Enon uplift. That is an area of relatively high seismicity. Are there any other places along this gravity low besides that in New Madrid, where there seems to be higher seismicity?

MR. RUSS: I am not sure I understand your question. Do you mean with this particular gravity low?

MR. RYDER: Yes. You said it extended in various directions, and I just notice on the path it seems to cut across two. You mentioned one area where there was higher seismicity, and it cut across the New Madrid area. I noticed that.

MR. RUSS: I don't know of any other areas associated with this gravity low where we have high concentrations of seismicity, but there are a number of structural and geographic features, including the Rocky Mountains themselves, which seem to be affected by it.

MR. JACKSON: This is a question I have been asked in the last several months. If we have learned so much about New Madrid, why do we need to do any more work there?

MR. RUSS: We still feel we can do quite a bit more in resolving better estimates of recurrence intervals, resolving better estimation of the actual faults as they are moving and the rates at which they are moving. Certainly work has not been done on ground motion in the area. Part of it has been due to lack of interest, and part, as Bob Hermann has pointed out, the lack of any strong motion data to model or work from. I think there is evidence we need to do more work, especially estimates of damage and loss.

MR. OKRENT: I guess implicit in Mr. Jackson's question was perhaps the definition of who is we.

MR. JACKSON: I was referring to the seismological geological community, not the NRC.

MR. OKRENT: Well, I am sure we will come back to New Madrid after lunch, so why don't we now take one hour for lunch? I will ask Mr. Hinze to please be back at 2:05 eastern time.

(Whereupon, at 1:05 p.m., the meeting was recessed for lunch, to reconvene at 2:05 o'clock p.m. of the same day.)

AFTERNOON SESSION

MR. OKRENT: Mr. Hinze, we are ready if you are.

MR. HINZE: All right, I am ready.

During the past several years, my colleagues at Purdue University, Larry Braile and John Sexton, together with Tad Lidiac at the University of Pittsburgh and Randy Keller at the University of Texas at El Paso have been investigating the possibility, and I emphasize the word possibility, of the northeast extension of the New Madrid tectonic feature and the seismic zone.

If I could have the first slide, please.

Because of the deficiencies in the seismic record, the historical seismic record, our studies have concentrated on the seismic, plutonic, tectonic mode which we are using the model of tectonic zones of weaknesses reactivated by properly directed stress patterns. As a result, our study has been largely a tectonic one in which we have closely integrated a variety of geological and geophysical results, integrating these into the tentative conclusions we have at this point.

For over a quarter of a century now, various investigators have speculated, and I probably should use the word fantasized, have fantasized about the extension of the New Madrid zone into the Ann Arch area that is roughly on strike with it and into the St. Lawrence River Valley seismicity. It is clear because of this high degree of speculation that there is no one overwhelming piece of evidence, so our approach has been to attempt to gather information from a variety of sources, little bits and pieces of information that we hope will provide us with the information about the extension.

Our conclusion is, we believe the overwhelming evidence indicates there is an extension of the New Madrid tectonic feature, and it does cross the 38th Parallel feature. The origin of many of our problems associated with the extension of the New Madrid feature into north of the 38th Parallel which is indicated by this variety of structures withiin the Phanerozoic crust really relates back to the map similar to this one, in which the Wabash River fault zone was called the New Madrid fault zone.

Basically, we have three questions, many detailed ones, and also correlative questions which must be asked. What is the controlling tectonic feature of the New Madrid seismicity? Obviously, we must get at that to answer Bob Jackson's question. Why should we study the New Madrid zone, because we know so much about it at the present time? I think it is quite clear. We must study it in order to do a better job of predicting the extensions of the New Madrid feature. We need more information on the tectonic zone itself.

And, does this tectonic feature extend northeast of the 38th Parallel lineament and into Southern Indiana? And a most important question we don't have all the answers to at this point, and are working on, what is the origin of the 38th Parallel lineament, what is it, and does it terminate in the New Madrid tectonic feature, and does it decouple it from the seismicity in the New Madrid area?

What is the controlling tectonic feature of the New Madrid seismicity? We have heard a lot from Bob and Dave and Tom and others about this feature. Basically, it is an aulocogen rift, and I will not repeat all of these types of evidences that we have that we can use in the study of the extensions. I want to emphasize the work of Hilinbrand, Kane, and others, which have indicated the areas of the specific location of the rift feature by virtue of correlative gravity and magnetic local anomalies, shown here in black on this diagram from Hilinbrand. These features have been very definitive in studying that, and we have used that same sort of evidence in trying to extend the New Madrid feature, again, the seismicity running down the center, and then the offset.

Very briefly, we have late pre-Cambian or early Paleozoic rifting. The Paleozoic and late Mesozoic subsidance, rifting, reactivation and intrusion in the Paleozoic and late Mesozoic to the present time with the present subsidance and the development of the Mississippi embayment analoxigen. Does the tectonic feature extend northeast of the 38th Parallel lineament into southern Indiana? Well, we have a host of evidence we can apply, and I think you will agree that many of these evidences tend to be rather subtle, but let's see what they look like, and we will look at some problems associated with geology, regional gravity, correlative anomaly, seismicity, and some recent seismic reflection surveys we have conducted under the sponsorship of NRC.

What we are working toward is a solution here that is a continuation is the feature we observed, a rift, and a continuation of the New Madrid feature. The strongest evidence for the extension in many people's minds are the Wabash Valley fault zones. They are roughly on line with the New Madrid feature. However, I do want to point out they are not oriented in the northeast direction as the New Madrid feature is, but they are oriented at about north 20 degrees.

I think if you think back to the slide that Tom Bushbach showed us, that we can see that quite clearly. Recently, we have done some seismic reflections, that is, we have contracted out some 12-fold CDP studies, and we have conducted 40 miles of line along two lines, 15 miles down here, working from the Hamilton County well, and the 25 mile line on either side of the Wabash River here, crossing what we think of as one margin of the extension feature, and the New Harmony fault zone, indicated by the single line.

The New Harmony fault is shown in a cross-section. This is some of the work sponsored by the NRC, and this is the New Harmony fault, which is one of the major of the Wabash Ridge faults. What we see here is the normal faulting, and incidentally, it is hard to map this with essentially vertical faults. Normal faults with vertical drill holes. And this is a very good diagram, because it shows where the control is, so you can do your own kind of massaging around.

Basically, what we see is this Horst and Grabens type arrangement, and where we follow a specific type of horizon across, and we see that the displacement on these faults is, and this is one of the more major ones, a couple of hundred feet, and notice the tendency to wedge out one of our seismic lines and that seismic line is right in here (indicating), and I should also point out on this slide that if you will note the seismicity here is in southwestern Indiana and is not directly associated with, except in a very few cases, with the Wabash River Valley fault system, but rather is off to the east, and we are going to be looking at a section right here.

I hope that you will understand that this is a very preliminary stack, and the interpretation that we present here will be burned immediately after this talk, so that we can start all over again.

(General laughter.)

MR. HINZE: But we have quickly tried to give you some insight into what we have come up with. This represents a west to east section of about five miles. I think you can see the disruption here associated with the fault zone, and I am having a hard time seeing this. This is a half a second here, this is two way, one, one point five, and two. We have a good reflection at the top of the Mississippi in limestone and another one down here at the basement surface.

I think we can see a very similar type structural pattern to the New Madrid faults that we have from the geological section, that is, it is a normal fault with the displacement rather minor, and in fact when we get down here to the basement, which is written here Mt. Simon, there is no disruption. At the most, perhaps, if you want to, put in five mils there, something around 50 feet, but a very small amount of displacement at the base surface.

This is another processing of that data, and focusing again on that area. This is about one mile wide again, west to east, and this is a better definition of those faults converging down to a single fault. It is much better to look at this, if you get up here on the side. We really thought about taking the slides at angles, because that is the way you look at these records. So from now on we will take angled slides rather than serial type slides.

There are other pieces of evidence regarding this feature. One is the geology, the basement geology, and I want to point out this is from some work put together by Ed Lidiac, a basement poll which sits on one of the correlative gravity magnetic highs in Lawrence County, Indiana. There is a paper dating back into the forties where the basement rock here has been studied and identified as correlative with Keeweenawan type basaltic rocks.

These are the kind of rocks that occur in the Keeweenawan area in Lake Superior.

Another interesting thing is, there are red clastics sitting on top of it, like those in the deep drill hole in the Michigan River basin, which goes into the rift below the basin, and others of these holes in our area in which we think there is an extension, many of these holes, as seen in some of the slides presented by Tom, do go into these red clastics. Another aspect of this is the regional gravity picture, and of course we have a regional positive anomaly.

I am looking here at upper contingent of 20. We have taken out the hash, and we are focusing in on the long wave length anomalies. The New Madrid feature down in here, I wish to emphasize the fact that this feature does indeed extend beyond the 38th Parallel lineament features and into this zone. We think that is important. We think it is important because the gravity high extends beyond the 38th Parallel lineament.

Very quickly, this is the gravity anomaly map that was first interpreted by Hilinbrand and Kane, and we believe that we can see a disruption or a dislocation of that zone up here where we have this intense seismic activity, and incidentally, where that low goes through, Dave, and we believe we can see an extension of that up to the northeast, and we will try to illustrate that.

This is the magnetic anomaly map of that area, and we see the bordering magnetic highs, and they do have correlative gravity features. They are obviously northwest striking features cutting across us, but you can see the lineation of those very clearly. You can't see very well from where you are this slide, and that is why I have put it up, but this is the gravity anomaly map, and the magnetic anomaly you have just seen with the correlative gravity highs extending up into this region (indicating).

Now, one would like to enhance that as much as possible, so we have looked at various types of processing of our regional gravity and magnetic maps. This is the Bouger anomaly map, and here we see the New Madrid feature. This is the Mormon -- the Rough Creek -- and this is what we believe is the northeast extension, and we are also suggesting, hypothesizing, and continuing to investigate an arm in which the Mississippi River flows up the center. This is what we call the St. Louis arm. It is also a zone in which there is seismic activity down the center of that arm.

This is massaged here. This is the 100 to 8 kilometer pass. We can see the gravity anomalies being pulled out nicely. If you want to look at blocks, here are some wonderful blocks to work with. And we can, I think, see the edge of the Grenville front down this type of portrayal. This is the first vertical derivative map, again, trying to look at some of those positive anomalies indicating the margins of those features. Presumably they are associated with intrusive and some of their associated extrusives that have come up along the margins of that zone.

This is a map in which we are distorting things to emphasize a feature. Here we have eliminated -- this is a strike reject filter on the gravity data. The northwest lineations are rejected, and we see the gravity highs, the gravity high on the lefthand side, the northwest side is much more prevalent. Of course, they are wiped out in a northwesterly direction for this feature. There are similar types of magnetic anomaly maps which I am happy to have you take a look at. We are also interested in the crustal studies, and we have crustal models, and we have done crustal seismic work from coal mine graphs, and this shows the results of two summers of effort in collecting data along lines extending from and between the coal mines.

There are some interesting features developing from that. This is a line extending over one of the gravity magnetic highs. The velocity of the basement rocks of that are considerably higher than that in the surrounding area.

This is the Wabash Valley average model. Some of the data are better constrained than others, but it shows a model that we can compare with other models in the area, and perhaps this is the normal crust. I believe Bob showed this previously. And this is the McCamy and Meyer west Mississippi embayment. The lower crust. And what we find in the Wabash River valley is a thickening of the lower crust in that area. This would go along, I think, with the positive gravity anomaly that we see extending into the zone.

Now, another piece of evidence which is kind of remote but worthwhile looking at is the work that some geologists are doing, and particularly Paul Potter, in looking at the big rivers, the rivers associating, major rivers that have lasted for long periods of time with aulocogens, and I want to direct your attention to the diagram in the upper lefthand corner. This is from Paul's Journal of Geology paper in which he has the old Michigan system river, which follows the direction of the old Wabash River. The Ohio River here is located for glacial reasons, by glacial deposits, but the old Mississippi River has been there for about 250,000 years, and this feature presumably for the same length of time.

It looks as if there was an old major river system extending northeast from the New Madrid zone.

MR. OKRENT: Mr. Hinze, you have about two minutes.

MR. HINZE: Thank you.

Another form of evidence is the seismicity. This is from a paper by Kovaks. We see the northeastern extension going up into the Anna area. We can also look at the seismicity, which seems to be, and here I am showing the positive gravity magnetic anomalies which we assume are the plutons. Not all of them will be plutons, volcanic masses, and these are the three arms extending off into the Madrid feature. The seismicity following here.

Perhaps a better piece of evidence is this diagram from Hadley and Devine in which we have shown the extension of the features together with their density of earthquakes, and we see a good correlation, except here in the Moormon syncline.

I might also say that this diagram, going back to our seismic record, and we are only looking here at about five miles, is that we have a very good reflector at about two seconds here which we assume may be the original basement. There is structure on here which is not multiples. There is some faulting activity. In fact, there is faulting activity that seems to be related to this disrupted zone.

I will move through some slides here. Does the tectonic feature extend northeast of the 38th Parallel lineament? Well, we have a number of pieces of evidence. We will skip over that. What is the origin of the 38th Parallel lineament? The 38th Parallel lineament has a number of lineations, disconnected lineations of various types of terminations of geophysical anomalies, mythologic changes, et cetera.

We are suggesting we have a crude alignment of rift arms, and let me show this briefly. This is what Pyle originally referred to as the 38th Parallel features. This is the Moormon syncline, the Roman trough, and into what we are calling the St. Louis arm, interpreted here on this map, so we would suggest to you that as a working model, with which to develop further lines of studies, that the 38th Parallel is nothing more than, if you will, a fortuitous alignment of rift arms, and I think the implication of that is that it does not terminate the New Madrid tectonic feature, and need not truncate the New Madrid seismic zone.

This is an attempt -- I threw this in in the last moment, because it shows the Appalachian front and the positive anomaly magnetic anomaly. This is some long wavelength, four degrees magnetic anomalies in this area, and I wanted to show that shows up not only in the gravity but also in the magnetics.

Well, to summarize, I think we have a lot of subtle pieces of evidence here which suggest that there are indeed arms to the New Madrid feature. There are correlative types of rift arms that we observed, and more modern rifts associated with the opening of the Atlantic. It suggests to us that there is no reason to terminate the tectonic activity at the 38th Parallel lineament. It certainly continues from the gravity standpoint, and also from the seismicity. Thank you.

(Applause.)

MR. OKRENT: Comments or questions?

Yes, sir.

MR. RYDER: Leon Ryder, NRC.

Bill, you said something very quickly, and you showed a very quick slide. I wondered if you would go over it. You said seismicity extends from New Madrid up through Indiana to Anna, and you threw a slide up.

MR. HINZE: Yes, that is Bob Kovaks' slide.

MR. RYDER: Could you show that again? I thought that was always a very questionable thing, and you seem to be saying --

MR. HINZE: It is a diagram prepared by a reputable seismic engineer. This was published in a publication that Bill Kovaks prepared when he was at Purdue for the Indiana Highway Commission. This is the regional epicenters, and it shows these kind of earth centers extending up toward the Anna area.

MR. RYDER: Is that equal coverage for all earthquakes over that whole area?

MR. HINZE: No, it isn't, because the seismic activity in New Madrid is not but it is the same data set from the rest of the area.

MR. RYDER: Do you think that is a demonstrated connection in seismicity between Anna and New Madrid? Do you think that?

MR. HINZE: I think there is a possibility of it. The evidence certainly decays in terms of the other geophysics as we move north of about 40 degrees, which is right about here.

MR. RYDER: You talk about the connection of the New Madrid extension. Do you have any feelings about earthquake potential with respect to the main part of the Mississippi rift structure and its extensions? Do you think there are similar earthquake --

MR. HINZE: It depends upon what is causing the New Madrid seismicity. If it is associated with the east-northeast stress pattern, related to a properly oriented tectonic zone of weakness, I think the southwestern area would have as much potential, but certainly the seismicity history does not indicate that.

MR. RYDER: I would really like to hear other investigators in this room who have looked at the extension comment on these ideas.

MR. HAMILTON: I will comment on it. First of all, it is my understanding, at least from what I have read --

MR. OKRENT: Excuse me. Could we get the lights again?

MR. HAMILTON: Bob Hamilton. From what I have read in the geologic literature, it is my understanding there are no mapped faults either in the Wabash system or the Illinois-Kentucky district that actually cross Rough Creek fault. I think this has been one of the arguments for not establishing continuity between the Wabash and the Illinois-Kentucky mineral district.

I want to ask, is anyone aware of any mapped fault that crosses Rough Creek?

VOICE: I just reviewed the last two reports for when I was preparing these two slides for the Wabash Valley, and both sets of investigators found them diminishing toward and not quite reaching.

MR. HAMILTON: So this faulting is late Paleozoic faulting, or at least it offsets Pennsylvania age rock, so that is an issue that must be dealt with in establishing continuity. The other thing, in the magnetic map in that area, the strongest feature is the northwest trending magnetic anomaly that is roughly coincident with the St. Genevieve, and I would be interested to know what your thoughts are as to the origin of that magnetic anomaly and when it formed, and it seemed to be the linear nature of it and the continuity of it might say something about the possibility of continuing the New Madrid feature to the northeast.

MR. HINZE: First of all, I think we have to be very careful about looking at the faults in the Phanerozoic rocks and determining what the extension of the New Madrid tectonic feature is. In essence, some of this faulting in the Phanerozoic rocks could be a red herring. What we are really interested in are the possibilities of movements along a tectonic feature in the upper crustal rocks beneath the Phanerozoics.

In terms of that northwest feature, it appears to have a great linear extent, and it appears to be associated with a gravity anomaly, and both of these would indicate that we are dealing with a major break in the lithology of the basement rocks, and some kind of a crustal province boundary. But certainly the seismicity as we see from, for example, Hardy and Devine's work, continues northeast of that.

MR. OKRENT: All right. We may well come back to this point. By the way, I should note, it has been suggested to me by someone in the audience that at least he would appreciate it if smoking were not maximized. I won't offer any opinions to people sitting behind the table, because there are too many chain smokers. So the comment only applies this way (indicating).

(General laughter.)

MR. OKRENT: Let's see. Mr. Herrmann is the next speaker.

MR. HERRMANN: I am Bob Herrmann from St. Louis University. Otto Nuttli and I and the other people at St. Louis University are deeply interested in the whole earthquake problem in the central United States, even extending toward the eastern United States, and we are interested in all aspects of the problem, from historical seismicity to present day seismicity to generation of strong ground motion, and just giving the right estimates of strong ground motions for construction sites.

We do have a seismic network, 30 plus stations, right now, and I am very familiar with the problems of operating seismic networks, both national and personal.

So far today what we have discussed is a lot of the why that earthquakes occur. So far, we have not discussed anything about what earthquakes do, but an important topic I would like to discuss right now with respect to local networks is whether the operation of these local networks is really relevant to siting a critical facility, especially since these are very expensive operations. So what I will do is, I will review what local networks can tell us, how good the local networks can tell us certain aspects about earthquakes, some future directions in using the data from local networks, and perhaps some future directions in which I would like to see the seismological instrumentation go.

So, as a point of perspective, if we can

MR. HERRMANN: As a matter of historical perspective, this is a plot of over 1200 earthquakes that are known to have occurred in the central United States between about 1800 and 1975. These are historical earthquakes, which means the earthquakes had been large enough to have been felt. This is from a report from Nuttli and Brale and it is in the NUREG series.

Obviously there are a lot of earthquakes that occurred, but that is just a point of perspective. If we center in on the central Mississippi Valley, this would be the plot of all known earthquakes in the 1800s. The New Madrid earthquakes are the large symbols, there, there and also one in New Madrid.

If we look at the first 50 years of the 20th century, we see a slightly different pattern, mostly that there are more earthquakes and more smaller earthquakes. You still see sort of a general pattern with everything near New Madrid. If you look at the third quarter of the 20th century, between 1950 and 1975, you see a lot of earthquakes in the New Madrid zone, some earthquakes in southern Illinois, and if you compare this to previous slides, you see this pattern of earthquakes in southeastern Missouri here in the Ozark uplift.

In 1974 the USGS sponsored the installation of a 16-station micro-earthquake network, and this slide shows approximately 1400 earthquakes located between 1974 and 1981. Obviously there is a very definite set of linear patterns in the embayment, but there is one other pattern that is somewhat interesting. It is the pattern of earthquakes on the flanks of the Ozark on the edge of the embayment going here.

What I would like to mention, in the last two weeks there have been 1400 earthquakes recorded by a swarm of earthquakes just northeast of Little Rock in this point here, and I have some seismograms on the wall showing some of these micro-earthquakes. So all the earthquake activity in the central U.S. is not necessarily in the center of the Mississippi embayment. Other earthquakes do occur.

Well, we can do other things with these earthquakes. We can certainly change the scale and look at different patterns. This is a plot of all earthquakes by magnitude which occurred between 1974 and 1980. Few earthquakes have been recorded and while located up in the Wabash Valley. The only distinguishing feature we see between these earthquakes and those down here is that these are perhaps a little bit deeper than those here.

Here we would expect focal depths in the order of between 3 and 15 kilometers, where some of the earthquakes up here located are at least 20 kilometers deep. This is all seismology can tell us at present about the difference between earthquakes down here and earthquakes up there and how that relates to the problem of where a large New Madrid earthquake would be in the future.

Those earthquakes I mentioned in Arkansas were 35 North and 92 West, so they would be where that arrow is right here (indicating), so they would be sort of on this trend (indicating.) We can do other things with this. Once we have determined magnitudes, we can start looking at the uniformity of the earthquake pattern. This would be a plot of all earthquakes with magnitudes greater than 1.5, this earthquakes greater than magnitude 2, this earthquakes with magnitudes greater than 3.

As was previously mentioned by David Russ, there seemed to be more earthquakes located between this point in Tennessee and New Madrid, Missouri, as opposed to the number of earthquakes in this particular region. That may be something apparent due to the detection capability of our network. If we look on this scale here, we only looked at magnitudes greater than 2. It is difficult to say the earthquakes are occurring more at this point than they are along the whole trend in Arkansas, so as a whole, they may not be uniformly accurate.

What do local networks do? The thing they are good at is locating earthquakes, and by locating earthquakes, I mean determining the epicenter or the latitude and longitude of the earthquake. The next best thing local earthquake networks can do is to determine the magnitude of the earthquake. There are two ways to get magnitude. One, the peak amplitude on the seismogram, put it into a format to calculate magnitude. Another way is to use the nature of the decay of the seismic code. That is very useful and we can get larger earthquakes. But the best thing we can do is find out where the earthquake occurred, and the second best is to decide how big the earthquake was.

The third best thing we can do, and this is not as easy because it depends on the distribution of seismograph stations, is the depth of the earthquake. This is a plot of some earthquakes I will show in the next slide. What I did is took all earthquakes within this rectangular box, I relocated them all, hopefully with more refined methods, and I will do vertical profiles along a line perpendicular to this linear seismicity trend and also parallel to the seismicity trend.

Here we are looking in a northeast direction. We see an almost vertical distribution of seismicity with a lot of scatter. Hopefully a plot like that, which has done a great deal in the San Andreas fault, will give us an idea of the depth of the fault if at all possible. We can't really do this everywhere. I can only do it here because I have got 1400 earthquakes. I can just search through the whole data base and get only the very best set of data and then relocate earthquakes. I cannot do this in Illinois where I only have ten well-recorded earthquakes.

The next thing a seismologist would like to do with local network data is to locate the focal nexus of the earthquake. Now, any earthquake along the New Madrid area, I make it 12, first motion data, good. It is very difficult to take 12 points on a plane and draw two perpendicular projections of planes on that in order to determine the focal. At points down in the New Madrid region where we have a good distribution of seismicity, we do have linear trends. We can use a technique called composite focal mechanisms in order to make up for the lack of stations by using many more earthquakes, lumping it all together, and hopefully a pattern will arise.

So those are typical things one would like to do with micro-earthquake data, with earthquake data, and these are important things. But I think for engineering use the engineers don't really care what the focal mechanism is, the engineer really wants to know, given an earthquake, what will happen at my site.

What else is there in seismological data to provide information pertinent to the engineering community? Well, if we look at another seismogram of an earthquake, what Aki realized in 1969 and which I was able to use in a recent paper is there is a dispersion in the frequency content of the signal. High frequencies arrive first, low frequencies arrive later.

This dispersion can be used to estimate an average Q for sheer waves in the crust. These waves are waves bouncing around in the crustal wave guide. Any earthquake would generate these waves; hence, we can determine this attenuation, this Q effect in the crustal wave guide, we can at least make an effort of what will happen going to a site.

So I was able to use the dispersion set ups and simple master curves and do some very non-exotic processing. You just need a ruler, perhaps a calculator, and some astra-curves and you can be a good scientist, and what I was able to do was determine a number for a quantity called Q.

Now, I had a graduate student who just finished a Ph.D. dissertation. We looked at worldwide short period seismograms, also seismograms of the long-range seismic measurements program which were made during the 1960s, and he was able to contour the variation of this crustal Q, the coded Q, for the whole United States.

We had known from previous studies by Otto Nuttli that in the center of the U.S. we have very little attentuation of seismic energy or of amplitudes with distance versus, say, the western United States and California, so the student was able to take all of this data and make a contour map like this.

Now, it wasn't anything parochial on our part, but we have the highest contour where our university is, but that is the way it came out. This map is interesting for one other thing, which came back to the design of the Grand Gulf Power Plant, and that is, given an earthquake in the New Madrid seismic zone, what would be the motion at that point in Louisiana where that power plant is. It just so happens that it is nice that this map is symmetric. This map always looks like the seismo-patterns for the 1811, 1812 earthquake.

Otto Nuttli found that the isosesimal patterns were elongated since he had no data here in an easterly direction, attenuated very rapidly going down. He only had an intensity of 3 at New Orleans, whereas an equal distance going to the east, they were intensity 5's or higher.

Well, this data we have from the study of seismograms, this quantitative data indicates that on the Gulf Coast there are lower values of Q and hence we would expect that type of attentuation for intensity or strong motion data.

One other thing that the student was able to do is we noticed that just looking at the data, the limited frequency range we had between a half-hertz and 2 to 3 hertz is that there was direct evidence of frequency dependence to the Q, so he was also able to make a not necessarily contour map but give us an indication of what is the frequency dependence of Q for the whole continental U.S.

It turns out that in the center of the U.S., the old continental shield, the frequency dependence of the Q is not very extreme, very small, .2 to .3, whereas up in New England it is about .3, in the western U.S. .4 or .5.

Now, this frequency dependence of Q has been published in the literature before by Aki. Aki also noticed that one of the implications of having a very high frequency dependence of Q in the western U.S. was that the spatial attentuation of strong ground motion, or response factor, would be independent of the frequency of the strong ground motion.

Starting with that, what Otto Nuttli did in a paper in a meeting in Knoxville last September is he made the assumption that in close to the earthquake, the response factor of the central U.S. or eastern U.S. earthquake would be exactly that of the California earthquake.

Then making use of the data we have acquired on the variation of the Q in the whole continental U.S. and also the frequency dependence Otto was able to go out to a distance of 200 kilometers, attentuate the response factor from in close to the earthquake to values we would expect in the central and eastern U.S. versus values we would expect in California.

A lot of the implications of that: first of all, the central U.S. and eastern U.S. has a much higher Q at 10 hertz than it does in California, so we do expect at one hertz for a period of 1^2 that you have a larger response for the central and eastern U.S. than in California, but the frequency dependence also comes into play as to what more we would expect in the response factor for larger distances.

This is one thing that developed out of looking at the total data in seismographs, strong ground motion in the central U.S. Up until two weeks ago the whole strong ground motion in eastern North America was limited by a lack of data. There were nine strong ground motion records recorded in the central United States. I could throw in two at Blue Mountain Lake, New York. There are a few now in South Carolina. There was one in Tennessee in 1970, three, I believe, and this is what a strong ground motion record would look like.

This is 10 kilometers away from a magnitude 4.25 earthquake. This is .5 g strong SMA, one with g sensitivity, and the peak ground motion here is about 7 percent g. All of the other strong ground motion records in the central U.S. between magnitudes 4.25 up to 5 were generally less than 10 percent g, closer to 3 percent g. It is not really the proper data base with which to do regressions or on which to base data.

I understand that the New Hampshire earthquake approximately a week and a half ago has generated at least 10 strong ground motion records fairly close in to and at larger distances. The swarm of earthquakes in Arkansas which has been going on for a week and a half has generated, triggered one strong ground motion at Lake Wakapello 200 kilometers away. That was a magnitude 4.5 earthquake.

We now have some more data. I think what we will be able to tie down by this data is at least the shape of the attenuations, but the distance, we will not be able to say anything yet on the basis of our limited data base as to how the strong ground motion should scale with magnitude.

Otto Nuttli and I have been putting together, using our seismological intuition, being careful about definitions and magnitude differences and source spectrum scaling between the east and west attenuation and everything else, postulating perhaps what a large earthquake would do in the eastern United States. These magnitude values in BLG probably would saturate. The largest one we would expect to see in saturation magnitude scales might be about a magnitude 7.

Going to New Hampshire, I would just make a prediction that if what we did was good science, if New Hampshire had a magnitude of 4.5, that anything out at 10 kilometers or so, the motion would be about 10 percent g on either of the horizontal components.

You can also do the same thing with respect to strong ground motion velocity. I have here four earthquakes and some scaling of what we expect peak ground velocity to do. Well, the particular question with respect to local networks is that local networks can give us more if we have the time, and incentive to try to get more out of the data.

I think with respect to the frequency dependence of Q and the attentuation of strong ground motion, we have fairly good contour maps for the continental U.S., between .5 hertz and 2 or 3 hertz. It is probably not and certainly not the frequency range of interest for nuclear power plants. They would like to know that information out to frequencies at least of 10 hertz.

There are a set of micro-earthquake networks established in this country which also have the benefit of being able to record digitally, so at least in a few points in the United States -- New England and Ohio, central Mississippi Valley, Utah, Washington, the whole California range -- I think we can get more information on the frequency dependence of this Q, and that whole wide frequency range, at least up to 10 hertz.

We have been doing some numerical modeling in the LG, and we can take this code of Q value, put it in the model and actually match empirical amplitudes of data. So our modeling techniques are coming along nicely.

Well, this is just a plot of the computer facility which the USGS provided us about three years ago. It has been a blessing. We can do a lot more of the data. It has also been a curse in that I have been spending at least two years programming it so we can do stuff with the data. We also lose manpower because someone must maintain this particular computer system. It is also challenging, but it is forcing me to think. Given the cost of the computer system, given the cost of the networks, given personnel costs, how do we get the maximum amount of seismological data?

Just an example of what this can do: we can bring up a trace on the screen. We can interactively pick the P wave, the S wave. We can push a button, locatge the earthquake, and just so that you want to give the analyst a pat on the shoulder that the analyst did a good job, as soon as you have located the earthquake it brings the thing up on the screen and it has an arrow -- not an arrow, but a circle drawn where the earthquake occurred. That was a nice little earthquake in the Wabash Valley.

Where do we go in the future?

MR. OKRENT: You have about 2 minutes.

MR. HERRMANN: I will do that.

In the future I am worried about what we are geared up to do is look at micro-earthquakes, we are geared up to look at seismicity patterns, hopefully to tie that to structure. Unfortunately, when the rare earthquake occurs in the central and eastern U.S. with a magnitude greater than 5, and those earthquakes occur every one to five years, or that New Brunswick earthquake with a magnitude of 1.5 to 1.7 which occurs every two years, we are not geared up to get good data, and there are people in this room who have done that by source parameters.

I mean with one good record or a few good records, we can get very tight constraints on the focal mechanism orientation. We can obtain good estimates of seismic material. We can get good estimates of the seismic function which would tell us something about stress dropping the energetics. We can also get good data on, given the earthquake, how do you propagate this or transmit that information to the particular site or construction site.

I have prepared a working paper, two papers. One I have given to the committee, and I have extra copies. One is entitled "The Relevance of Regional Seismic Networks With Siting of Critical Facilities," and the other is a working draft, "Future Directions in

Regional Seismology.

As a stopgap measure because I am interested in the whole problem, I would like to bring to your attention that in the U.S. there have been historical earthquake laboratories. These are literally falling apart. The clocks in many places no longer function. When the silver scare came up, prices increased substantially two years ago. People were actually thinking of getting rid of the photographic recording and converting to pen and ink recording, which in my mind, knowing the people who would be doing that and their capabilities, would also mean the instruments would be no longer calibrated, and to a seismologist interested in studying the earthquake quantitatively, it would be a great loss.

Technology today in the micro-computer field and everywhere else has changed significantly. I think it is feasible now to upgrade our whole recording base in the central U.S. or the whole eastern North America for a minimum amount of money by just putting a digital recorder and hooking it into the existing long period seismometers.

Many of you in the audience know there are problems with non-linearities on the seismometers, but it would give us broad band recording. I think some non-linearities could be filtered out. We would then have the ability of on-scale recording for these earthquakes when they do occur, so that when they do occur, we could then provide our sponsoring agencies and the engineering community with as much detail on those earthquakes which is conceivably possible today.

Two more slides.

Here is a portable strong motion instrument for studying small earthquakes. Here is an actual vertical component of a strong motion record done on the Mississippi River embayment. Here are three synthetic ground velocity seismograms. These are for three basic earthquake sources, and seismologists know that we take a linear combination of those three so we should be able to model an earthquake.

I would like to point out to you, though, if you look at the phases on the theoretical one, you have one, two, three, four phases. There are about three or four phases on those records. It seems quite possible, given good data, we can literally do an inversion of poor source parameters. It also seems possible in my mind, and I am pushing my students and myself to do it, that given micro-earthquake data from regional networks which is digitally recorded, we could probably low pass filter that, get a nice, coherent signal at the lower frequencies, and then use our analytic techniques to routinely give us focal mechanisms, seismic moments and perhaps source steps and corner frequencies.

We cannot get focal mechanisms from local networks today. If you wanted focal mechanisms in the Wabash Valley, instead of having eight seismographic stations they would have to have 80, and then they would have to have big technical staffs. I think today, the way theoretical seismology is going, if we just had a few good good recordings we could do probably as much, if not more, than these local networks.

So thank you very much.

(Applause.)

MR. BUSCHBACK: You just said you couldn't, but with the limited data, could you compare seismic activity or at least the events you have been able to run solutions on in the New Madrid area with the Wabash Valley area with regard to depth or focal points?

MR. HERRMANN: Tom Buschback is asking what we have been able to do so far in the Wabash River Valley versus New Madrid and that whole problem with the northeast extension. We have done very little. We have located the earthquakes, but I have a feeling that what is happening on the Wabash Valley is not that earthquakes are occurring on one fault, therefore I couldn't use a composite focal point technique to put it all together in a picture.

The zone in one of the maps in New Madrid -- if someone would kindly turn the projector back on -- every time I look at data and try to acquire new data at New Madrid, I become amazed by the complexity of patterns of seismicity, mostly because I am trying to get more than my knowledge of the earth will permit me to obtain.

What I would like to call your attention to is this pattern of seismicity between New Madrid, Missouri and Ridgely, Tennessee. In the past three summers we have had one month micro-earthquake survey in Ridgely and then another right up near New Madrid this past summer. We, together with the University of Wisconsin and U.S. Geological Survey put about 30 instuments in this particular zone.

Every time I plot up those epicenters and try to look at the spatial patterns, the three-dimensiontal spatial patterns of those earthquakes by looking at vertical projections or by making two-color 3D images and getting my 3D glasses on, no pattern really arises. It is a very complex zone in here. You could look at the map and say, hey, we have got something northwest-northwest-northwest. Sometimes you swear that for the people with the Cotton Grow fault, I sometimes swear I see something in a northeast direction there. It is very complicated. And given all the data here I can throw away, just to take the best data to see what I would come up with in Wabash where I have very little data to begin with, it is very difficult.

So I think the thing is I would like to see it go that given one earthquake which a magnitude 3, which occurs in Wabash Valley about once every year, if I had good broadband data for that earthquake, if I analyzed in three or four stations that data, I could get the information I need for each earthquake. Technically the science is there. I think there data are lagging.

I might mention, just for upgrading these long period instruments you could probably do it for $5,000 a station, and half of that would be for a good clock.

VOICE: Bob, I will try to pin you down on what Bill Heinz said. I would really like any other people to comment on it, and I see that Otto Nuttli has walked in, but Bill Heinz has said he thinks seismicity supports the extension of the seismic zone from New Madrid to Anna, Ohio. Maybe I am misquoting you.

MR. HEINZ: North of the 38th Parallel. I didn't say to the Anna area.

VOICE: But you mentioned Anna, Ohio. You did. I just want to draw some comments from the people here who have studied the region to find out what they think about this.

MR. HERRMANN: This is Otto Nuttli's earthquake catalogue, and you do see that northern Indiana has nothing, fine. So therefore, if you look at where all of the black spots are, here it is obvious there is something perhaps going through there. Now, these are all known earthquakes. That means you could have the smallest felt earthquake of magnitude of 3.5. Otto Nuttli in the report with Paul Pomeroy in the NUREG series plotted this data in a slightly different manner.

He plotted it with earthquakes of magnitudes greater than 4.5 or 5. In other words, those large enough to cause structural damage. And a different pattern arises. There are other things, too, so when we start talking about why are those dots there, if we are talking about very small earthquakes in a catalogue we must be careful about the validity of the catalogue.

Ron Street, University of Kentucky, is a consistent person who will dig into the catalogues looking for errors. There was one earthquake in the Otto Nuttli catalogue which got into the NUREG report, a very small earthquake, and Ron sent Otto a newspaper clipping, and what happened was a railroad car blew up. Is that right, Otto?

MR. NUTTLI: That's right, Bob.

(Laughter.)

MR. HERRMANN: We are a team, yes.

(Laughter.)

So we are trying to base our design decisions on earthquake catalogues. We have to be very careful about the data, and if it turns out this pattern going into Northeast, apparently over to Ann, Ohio, those were the little earthquakes, we had better be very careful about the correctness of the catalogue. We can't go to U.S. earthquakes. We must go back to the original sources. That hasn't been done. It is a very time consuming thing. You almost need a seismologist to do that who knows what he is looking at, but it seems to be something more akin to what a history major would like to do. Seismologists would rather do other things.

So what we have done in the past is we have had catalogues which have copied catalogues, and so if any errors have crept in, they have stayed in, so there are problems. I am just pointing out the problem with the data set. I won't conjecture, Lee, on whether it is true or not.

VOICE: What about the micro-earthquake

patterns?

MR. HERRMANN: Micro-earthquake patterns over that region? We have not recorded anything. Our network over there, the stations are sitting on nice glacial tills, they are logging stations, so the earthquakes are 3's. But as far as I recall, our network does have the capability of locating things. It is sort of interesting that we have not picked up anything there at all. This is over seven years of recording. So if there is an apparent pattern, it is certainly not due to large earthquakes.

Any other questions? Yes, sir, Carl Stepp.

MR. STEPP: Bob, I would like to raise a slightly different question. I wonder, in looking at the Mississippi embayment for a lot of years, if people aren't a little bit too much influenced in their decisions or interpretations of the extension of this zone by the relationships among fault structures in the near surface.

It is quite apparent that these fault structures have been developed by numerous different episodes of activity over long periods of geologic time, and what I have seen here today is a considerable refinement in the geophysical information on the deep crustal structure of the region, and it seems to me therein might lie the answer to the extent of the seismic zone.

There seems to be some difference in interpretation of that deep crustal geophysical data, and I would like to hear comments from people.

MR. HERRMANN: I think I will start off because I was a physics major and got by with the minimum number of geological courses, and from the little geology I know, which is from western geology, if there is an earthquake there is a fault. That's not the problem. The whole problem, as Carl Stepp knows and the staff knows, is that, gee, there is no evidence of any geological structure being associated with a known large earthquake in the eastern U.S.

The problem with New Madrid is just right at New Madrid itself there are at least 2000 feet of alluvium, and I literally mean water-saturated alluvium. We have to use geophysical techniques to go deeper. There has been a lot of progress with, well, Dave Russ has stretching. That indicates strong ground motion rather than perhaps folding. I will be in Colorado for the next six months.

MR. OKRENT: If I may, I will suggest we move into the general discussion on the subject of the last four papers, and if anyone wishes to, he can bring in topics touched on earlier in the morning. So let me open up the floor. We have really been in it on the last couple of questions, but I will formally open it up.

MR. MARK: This is a totally non-technical question. You were just mentioning it would cost about $5,000 to put an existing station into a condition where it could get the sort of data you think could be used. What does operating a seismic net of a dozen or 20 or any given number probably cost you to accomplish?

MR. HERRMANN: You might ask what it costs the USGS and what it costs us. It costs us a lot less because we get a lot less. Right now we are giving -- I will just give you the figures for the USGS, and including the stations we are supposed to be putting in for a 50-station network of USGS and NRC funding, it is about $260,000.

MR. MARK: That is the installation or the year?

MR. HERRMANN: Just for the operation and analysis, the maintenance of the network and analysis of the data. So most of it is personnel costs. The hidden costs in operating a network the way the networks are configured today are the telemetry costs, and that is a big problem with respect to the funding agencies, tiven the fact that AT&T has just revised all of their long line rates.

So the hidden costs, I am not sure, I am guesstimating, in the Wabash Valley for eight stations is perhaps $20,000 a year. So you can get eight seismic signals on a telephone line and you divide that into 50 stations, and that means we need at least six phone lines. That is $120,000 a year. So if we add all of that together, perhaps just for us, $400,000 a year. So there is a lot of money in running networks.

MR. MARK: Thank you.

MR. HERRMANN: The other thing I would mention as far as upgrading stations, you would use the sensor that was there already, just put a digital recorder, gain ranging electronics, and a clock. I may have an underestimate there, but I don't think I am off by very much, and you only have to do that to about 20 stations in the whole U. S., so that is $100,000.

Bob Hamilton is asking about worldwide stations. There is a program within the Geological Survey to upgrade the worldwide stations to digital recording stations. I would in my mechanism include the worldwide stations but hook onto another signal coil and get data in another manner as opposed to how the surveys do. I may have a slightly different philosophy to recording, but I think that is the way to go to get the data in the future. We need the data, and we cannot afford to lose any earthquakes that cause damage.

MR. OKRENT: Let me pose a general question to anyone brave enough along the lines of Leon Reiter's question, but I will put it this way. Do we care whether the New Madrid geophysical features in fact extend out through Anna and the St. Lawrence? If so, why? Would it change one's estimate of the likelihood of earthquakes of increasing severity in the regions northeast of the currently well-defined portion of the

New Madrid zone? If so, why? And is there any kind of research that would change your current opinion about my set of questions? Okay?

(No response.)

MR. OKRENT: We have enough experts here. I hope we can find a few volunteers.

MR. BUSCHBACK: It is not bravery, but something else that lets me speak on this subject. I personally think that the Rough Creek Grabens and the sharp dropoff combined with the northwest lift gravity anomaly decouples the seismic zone from the Wabash from the north. However, in defense of Bill Hinze's statement, I think our entire program would be a failure if we did not try to recognize geologic, geophysical features with which seismicity is currently associated, and then come up and say we have another one like this but there is no particular great amount of seismicity, and pass it off as that. We might just as well then go back to historical seismicity.

I think, yes, in answer to your question. You asked another deep and astute question following that, and I knew right then I wouldn't try to answer it, so I have forgotten it.

(General laughter.)

MR. BUSCHBACK: But I think, yes, we should be alarmed. Bill, when he told me about this, said he is not raising a red flag but a yellow flag.

MR. BAROSH: In regard to your connection of New Madrid to the St. Louis, I would say if you look at the frequency and historic seismicity, it is not really if there is a connection, but how it is connected, because there is a band of seismicity going up that whole way, but it is concentrated in different spots. In terms of geology, it means that there does not have to be any direct connection with geologic structures between these areas. It is just, it is an overall weak zone, which we are trying to understand. In many cases in geology we have en echelon features running along a zone. The zone is north 45 east, but all the particular features along that zone that are active may run something like north 20 east. Yet they are all part of this one zone of weakness, and this is the kind of thing that we are attacking and needing to know.

Around the Anna area there, there is a good north-south trending buried river. There is a lot of activity concentrated over that buried river. Perhaps there is some north-south structure along that. That has been suggested by some who have worked there. The same at Attica. There seem to be north-south structures predominant there. Attica is on the same line as you come along from Anna and New Madrid.

These structures may be in response to one overall force through the area, but not necessarily a single, continuous structure. That is the kind of thing we need to understand, and why are there these particular buildups in these spots?

MR. OKRENT: Let's see. Let me elaborate a little bit on what I think I have been hearing. It has been at least suggested perhaps the Charleston earthquake could occur elsewhere than Charleston, and I think it has been suggested that perhaps the New Madrid earthquake could occur elsewhere than New Madrid. So I am trying to see whether there is a suggstion in the kinds of things Mr. Hinze was talking about, and things that have been said in the past, of course, concerning the possible extension in a northeast direction as to whether one has to consider that there is this kind of zone of weakness, and then perhaps taking Mr. Chinnery's comment about how here and there things are locked, and then they break, or whatever, that this whole zone going up the St. Lawrence is a potential region where you might in some future time have something not unlike the New Madrid.

If this is at least as tenable a hypothesis as some of those being made concerning Charleston, how could we investigate it? Can we investigate it really in any meaningful way? In other words, is there some kind of research that really would shed light on it? Do we have to in fact just know what the stresses are, or what, or is it just not something that is subject to sufficient definition that we should hope to be able to use such research to feed into engineering judgments? Where does this all end up?

So let me expand my question with that kind of background.

MR. PHILBRICK: Dave, if you don't tie those things down to some sort of structure, you get into some ridiculous situations such as occurred in that Green County thing on the Hudson, where they took the earthquake and moved it over towards Green County.

MR. OKRENT: I don't know what is ridiculous.

MR. PHILBRICK: It is ridiculous because it is on a structure, and the structure was established.

MR. OKRENT: Because I remember people arguing very strongly that the Charleston earthquake could only be localized at Charleston or at best there was some structure that maybe existed heading toward Charleston. That is not that many years ago. And you heard it, too, I think.

MR. PHILBRICK: Oh, sure, it is all clearly stated. The thing had to be in Charleston. It could be nowhere else. Which was a great thing, because if it didn't, there were a whole flock of structures, plants that were involved. So I think you have a reasonably good region for continuing your studies.

MR. OKRENT: In any event, if someone can help me, I would appreciate his comments.

MR. HINZE: Bill Hinze, Purdue.

I think it is worthwhile to look at the history over the past five or ten years in terms of the New Madrid area. Really, about ten years ago, most of us, all that we knew was that there was an earthquake zone there possibly having some type of northeastern extent, and we have heard here today, I think, a lot of evidence which gives us a pretty good feel for why those earthquakes occurred. They are not perhaps specifically at New Madrid, but along that feature.

Originally, the information that localized that was gravity magnetic information, and those of us who play with that data know better than anyone else the ambiguity of that data, but it does give us an area in which we can concentrate our attention and efforts, and that is what was done by the USGS in their reflection seismic on the original program, and now the followup program.

As we tried to look at it from the northeast into Indiana, we were on the brink of insanity, the realm of insanity, because we realized the features were very subtle, and those of us who have played with rifts for a few years know that rifts take on a wide variation of signatures, even where they occur in relatively recent times, and here we are looking at something that has been chopped up and beat around, and we realize that we must be prepared to interpret very subtle features.

But what we have to do once we have interpreted that gravity magnetics, what we should have to do is to use that to concentrate our energies and efforts on more direct means of investigation. It was that gravity and magnetic work which had us first lay out a seismic line, working from the coal mines. That was really in the wrong place, because we hadn't interpreted the data yet, but we had to get going with our work. In the subsequent year we were able to detail it better.

Then we also realized that we would not and you people wouldn't believe and none of us would believe it until we had more direct evidence. That is why we have looked for such evidence as the red clastics. The red clastics and the mid-continent, we think, provide a good marker for rifting processes.

Now, not all rifts will end up with red clastics. They are going to end up with volcanics in them. Some of them have been stripped off, eroded off. Well, what else can we do? We were interested in getting more direct information, so we have conducted 40 lines of seismic reflections. Forty miles doesn't get you much, but we are very appreciative of the opportunity to do that.

Now, that interpretation that we have on that is just very preliminary, but we find it terribly exciting and, we think, important that in southwestern Indiana we have a very excellent acoustic impedence boundary beneath what we think is the Paleozoic, the bottom of the Paleozoics. This provides us with the opportunity to directly look at the possibility that we have a rift structure, and what we need to do is extend that further to the northeast to complete that interpretation, extend it further to the northeast, and then to look at, at Mike Chinnery says, perhaps we have the intersection of fault zones.

I am down in print, and many other people are as well, that the Grenville front, which is a major tectonic front, extends out through western Ohio, and if you want to be a straight line geologist, you can draw a straight line between the New Madrid area and the

Grenville front. I think it is very important to us to say, is Anna going to expand on it, and if it is going to expand, if the area of seismicity is going to expand, in what direction will it expand? Is there a chance of it occurring to the southwest along the extension or the north or south along the Grenville front? These are things we must determine.

So, I think you can lay out a conceptual model based upon our potential field measurements which are relatively cheap, and zero in on it with more direct geological information from drilling, from seismic reflections, and that is the place to start.

MR. OKRENT: Mr. Sykes?

MR. SYKES: Yes. Lynn Sykes.

Let me comment about this question of the St. Lawrence to New Madrid zone. In some of our work in western New York and northern New York, it has been aimed at trying to look at features there and focal mechanisms, and certainly what we find is that there is no continuous structure in terms of the small or moderate sized earthquakes. For example, one place along there of activity in northern New York state would include the scene of the Cornwall earthquake of 1944. That zone appears definitely now to be a north-northwest trending feature. It is backed up by about ten focal mechanisms of earthquakes, so it is not something that is aligned along the St. Lawrence, nor is it something that is just as a point source along the St. Lawrence.

If we go over to Attica in western New York, we are seeing there a rather concentrated source, at least some of the activity related to one specific fault. One thing that has not come up here in the context of this meeting so far that I think has been a very important result of the networks of the last five or ten years is that we are seeing some areas with exceedingly low seismic activity, right down to the smallest earthquakes we can detect.

Much of the central part of New York State and the central and western parts of Pennsylvania are exceedingly quiet, and this certainly has a big impact on reactor siting. The region along the south shore of much of Lake Ontario from ten years of our network data, much of that appears to be exceedingly low in activity, so that obviously has a consequence to some reactor sites there, to some major studies located in that region.

MR. CHINNERY: I would like to add one comment to that. I am constantly reminded of the Cape Ann area, where in the last ten years we have had remarkably little seismicity, and yet historically it is quite clear that is a center of activity. If that is possible, one gets very worried about the business of trying to connect zones together. We have an awfully short period of recording.

MR. SYKES: Let me come back to that. From the focal mechanisms that have been done and the stress measurements along the eastern margin of the U. S., there is an indication that the data are not too good yet for New England, that there is a common stress pattern, a fairly common focal mechanism pattern that would seem to prevail along a fairly long region there, and would probably go up and include Cape Ann, but that is something on which we need some more data to see if that is the case.

If a Cape Ann earthquake were to occur in the middle of New York State, I would be much more surprised than I would be to an event that would occur somewhere within that Atlantic margin zone.

MR. CHINNERY: (Nods affirmatively.)

MR. NUTTLI: My name is Otto Nuttli, from St. Louis University.

I just arrived a few minutes ago, and as a result I haven't had the benefit of all that has been said earlier in the conference.

VOICE: That sounds like an advantage.

MR. NUTTLI: But I do want to express my opinion about the extent of the New Madrid fault. Based upon historic seismicity and present day earthquake activity, in my opinion, the fault zone begins a little bit northwest of Memphis and extends up to about southern Illinois.

Now, I don't mean to say there are not earthquakes which occur on a line which is an extension of that or a line of geological features which are an extension of that, but the size of the earthquakes and the recurrence rate of them are entirely different from what I call the New Madrid zone, and what you see to the northeast, the Wabash Valley has a potential for fairly large size earthquakes. I will give a number later on in one of my talks. But damaging earthquakes, certainly.

But beyond the Wabash Valley, I think you can only expect relatively minor earthquakes, if we can use seismicity at all as a judge of what will happen in the future.

MR. ZOBACH: Zobach, USGS.

I would like to point out that the reflection profiles that were shot in New Madrid found quite a bit of faulting in the Tertiary, and over one and one half kilometers of offset in the pre-Tertiary Paleozoics along the major fault zone that presumably major earthquakes occurred along, and that is rather diagnostic, and we don't see features like that outside the zone. We don't know. We haven't done profiling to examine the extent of the zone, but we know from the available data that offsets of that magnitude are probably not prevalent outside the intense zone of seismicity.

MR. MAXWELL: May I ask a question? Dave, I am a little confused. We have heard that the boundary of the New Madrid structure were in dipping at something like 10 to 12 degrees, and you say that along one of these major boundary faults you have a kilometer and a half of displacement. What does your fault look like? Is it steep?

MR. ZOBACH: The fault I am referring to is one we see on a seismic section crossing the seismicity that goes down the center of the rift valley.

MR. MAXWELL: Not along the boundary.

MR. ZOBACH: Not along the boundaries. It goes right across the main zone of seismicity. What is surprising about that reflection line is that the Tertiary units are not disturbed very much, but in the Paleozoics, we have sort of a shadow zone where the fault is but on either side we see about one and a half kilometers of vertical offset.

So, it is clearly a major fault right where the seismicity is. I don't think -- I am not very sure, but I don't think I know of any other similar offsets in the surrounding regions.

MR. MAXWELL: Is it a high angle fault, quite high?

MR. ZOBACH: Yes. According to the focal mechanisms and what we see, it looks like a vertical zone, and probably a strike slip motion.

MR. POMEROY: May I ask a question? How many similar rift structures do you have with the kind of seismic coverage you are talking about here?

MR. ZOBACH: Well, none that I know of, and of course that is why we are trying to characterize New Madrid, so we have criteria which we can use to investigate other rifts of this sort.

MR. POMEROY: But there is a potential of a number of other rift structures in other places where we could have these kind of offsets, and we simply don't know about them.

MR. HINZE: A good example of that, Paul, is in the Rough Creek Graben. I think some of us have seen some oil company data over that data, the seismic reflection work, and there is a beautiful trough, and yet this is not a seismically active zone.

MR. MARK: You mentioned oil company data. That is drilling, I assume.

MR. HINZE: No, this is seismic reflection.

MR. MARK: Do people such as yourself have easy access to oil company core drill data and seismic data?

MR. HINZE: If you have a photographic memory, you could hang onto it as they whip it by you, and then you can't say anything about it in public. I mean, you can't use the data.

(General laughter.)

MR. HINZE: We have some seismic data reflections from southwest Indiana from one of the oil companies. We used it to help locate our seismic reflections. But we can't say anything about it.

MR. BUSCHBACK: Many of the states have laws which require the records to be turned in. Arkansas does not, unfortunately. But within a year, plus the fact that we can get a team like this together when an oil company drills below the Knox, they have to come to one of us to fix tops for them, to hold their hands when they start wondering what is happening. So one of the group usually gets to see the subsurface data. That is not seismic data, but bore hole records.

The ones in Mississippi County are held

confidentially now, and probably will be for quite a while, but in any other state we do have access.

MR. MARK: They are mandated to be public?

MR. BUSCHBACK: Within a year. Various states have different rules. This is only drilling now, not seismic.

MR. GIESE-KOCH: Gus Giese-Koch of the NRC.

I would like to come to this point from another standpoint. I came here via the aerospace industry. We have seen from all of the money we have seen from aerospace how much benefits we reap from the research done specifically for the aerospace and used for other research. For instance, the digital units we have right now used in seismology were developed through aerospace. So what we should look at with new research is cost benefit, and in that respect we should not only look at cost benefits for nuclear plants where we say that for every day that the nuclear plant is down, let's say you have a 1,000 megawatt unit, it costs $1 million a day, but also, what about all of the buildings that we have to build?

The uncertainties we have in seismology are such that we overbuild. Were we more certain of where and what earthquakes are like, we could very well be constructing in quite a different manner, and the cost of such versus the cost of the research could possibly be written off in that respect, rather than just looking at what the cost of research is right now, and in that respect I think we should be willing to put in more instrumentation and be more certain as to where and what earthquakes do, and in what manner we can explain them.

MR. OKRENT: I think that may be so but in a sense that would be a question for the President's science advisor, since he must think more broadly than the NRC.

Well, I guess that we had best move on. I am going to suggest we wait a while before our break, since dinner is quite a few hours away. Why don't we begin on Charleston southeastern U. S. portion, and take a break after the first one or two talks?

Mr. Bollinger.

MR. BOLLINGER: I am Gil Bollinger, from Virginia Tech at Blacksburg, Virginia.

By way of introductory comments for an overview, we now have approximately 100 seismograph stations operating in the southeastern U. S. Now, this represents a significant, perhaps a tenfold increase of monitoring capability during the past decade. This increase in monitoring capability has not, however, as you all know, been uniform in either space or time.

The largest data base developed to date applies to sites of the regions' two largest shocks. Eight years of monitoring in South Carolina and four years in Virginia. We will be hearing results from those two areas, but it is important for me to note here that there are two other areas of historical and modern seismicity that have only recently been instrumented, and we will not be hearing reports from them. That is the Tennesse-North Carolina border region and the central Alabama region at the terminus of the Appalachians.

Also, our results from central Virginia are preliminary, and because of time constraints, then, I will also not comment on them.

The principal results to date derived from the southeastern U. S. network are essentially three. First, the seismic zone defined originally on the basis of historical macroseismicity, that is, fault events for some two centuries are unchanged spatially on the basis of these short-term microseismicity during the past decade. That is not a redundant statement in the sense that they remain unchanged, because we put the networks where the historical seismicity was. I am referring to things like the South Carolina network has been adequate to define that there has been no microseismicity above some level, say one magnitude, one or two off-shore, from Charleston, which has a bearing on the Blake fracture zone.

Also, in central Virginia, our network has been adequate to show that we have recorded no microearthquakes in the Virginia coastal plane, where historically there were none. They seem to be combined with the Piedmont. So that is what I mean by no change spatially.

Secondly, there have been some specific geologic source models formulated for the various important sites in the region which we did not have before.

Third and finally, there have been instances or cases of focal depths being estimated with reasonable precision. This is new and very important, obviously, geologically.

With respect to that latter aspect, it has been observed in both Virginia and South Carolina that recent earthquake foci extend to crustal depths of 15 to 20 kilometers, thus seismogenic structures in the region can be significant crustal features. With respect to the geologic source models, the Charleston, South Carolina, area has had two markedly different models. Wentworth and Merker Keefer, Tar, Ray, and others argue for modern reactivation of steeply dipping faults. Conversely, Barron, Hamilton, Seeber, Armbruster, and others have proposed the source of the 1886 shock was movement along a sublar detachment fault. For their model, the modern seismicity would be secondary and associated with steeply dipping faults that splay off from the master detachment fault.

We will be hearing obviously at this meeting directly from several of the Charleston area investigators. So, for the remainder of my report, I will consider the results from Giles County, Virginia.

With our results to date, we have formulated a geologic source model which has the following characteristics. There are some 14 to 16 earthquake hypocenters configured to present direct instrumental evidence for a tabular distribution centered near Perrysburg, Virginia, the presumed epicenter of an intensity 8 shock in 1897. That zone is some 40 kilometers long. It strikes northeasterly and has a nearly vertical extent over a depth range of from five to 25 kilometers.

Geologically, the zone is entirely within the basement beneath the Appalachian detachment fault there and at an angle of some 20 degrees with the strike of the tectonic fabric with the host southern Appalachian.

This is the first instrumental evidence for a seismically active fault or fault zone in the southeastern U. S. that does not parallel the trend of the surface geologic structure.

Now, I realize that 14 hypocenters are a very small number indeed to formulate such a specific model. However, the zone has experienced seven felt earthquakes over the last two decades, and thus the seismic energy release, while low, has been persistent, and has exceeded the microearthquake level. So, we will go to the slides, and I will try to convince you that we do have a reasonable interpretation, at the same time, perhaps, present an example of a detailed network study.

Now, different from Bob Herrmann, I am not willng to trade my network modeling for one long period vertical digital. I think there is much to be gained yet from multistation monitoring. May I have the first slide, please?

This is a station location slide as it exists roughly now. The solid black dots indicate a single station. The gray circles indicate multistation concentrations. I don't have all of the stations here or in Anna, but this is essentially the distribution in the southeastern U. S. in Charleston, South Carolina, in Virginia, and now we are getting them in the Tennessee, North Carolina area.

This is a seismicity map for the last four years. The solid circles here represent single events. Most of these are microearthquakes, and the open triangles indicate multiple earthquake occurrences. Again, a small percentage, just a few represent felt events, but this is from the southeastern U. S. Seismicity Bulletin. It represents the last four years, and the purpose here is to show the spatial distribution is as published some years back.

This presents the geographic and geologic location of Giles County, shown here in the solid white. It is betwee the Blue Ridge and the plateau regions. It is in the valley and ridge province of Virginia. This is an overview of the Perrysburg area, the presumed epicenter of the 1897 shock there. This is Angel's Rest Mountain, which was erroneously reported early on to have split open. Of course, that had not occurred. Our Narrows station, which is the central station of our Giles County network, is located in this valley, about here (indicating).

I will use that station, NAV, at Narrows. The town of Narrows is here, as a key, from slide to slide, particularly the slides where I do not show much other geography.

This is the Virginia Tech network, and I will be concentrating specifically on results from this five-station network here with the central station at NAV. We are in the process of installing four other stations now to make more of a ring about this. One of those is in right now, here, and the other is being installed.

These other network data we will report on at a later time. This shows a magnitude 1-1/2 earthquake, recorded by two seismometers on the same pier at Blacksburg. This is a worldwide standard seismogram, and this is the high frequency emphasis seismograph from our network data.

The micro-earthquakes being rich in high frequencies, if you emphasize them you can get impulsive P and S phases. Now, it is the impulsive nature of the S phases and the fact that we devoted an extraordinary amount of time and effort in developing a velocity model that had both P and S measured velocity using multiple sources, quarries, earthquakes and others inside the Giles County network to develop a velocity model specific to that locale.

So given a good velocity model and decent signals, then we test our capability to locate events within the network. And this was a test where we monitor with the network stations blasts occurring inside the network, pretend they were earthquakes and then locate them.

Then we go to the field with a seven and a half minute Topo sheet to get the shooter for the blast and spot the two locations. This is the town of Perrysburg. This is our Narrows station. They are constructing a bypass around Perrysburg, so there were two relatively large blasts, indicated here by the star, and we called them A and B. Our locations using the error-ellipse program are at A and B here, and you see the 68 percent confidence ellipses about the epicenters.

As a subsequent and later repeat of this experiment, there was another blast several months later that was located at C here. It was a smaller blast in the sense that they used less charge. We had a lower signal-noise ratio and we expected and got a greater error in our location here and in its error ellipse.

To be more specific, here's the errors for the three blasts in terms of the epicenters. The actual difference between these two was 500 and 900 meters, and then 2 kilometers. And the ERH, the horizontal error from the error-ellipse programs, gave these numbers. So these are excellent.

Now, we couldn't test our velocity model at earthquake focal depths, and from this test we had a trial focal depth to start our probing with. And then the solution we started it for -- we ended with .5. We started at 10 and ended at .10. Our ERC's here are very large, but that is because we did not have a station close to the blast itself, so that we had no control in that sense.

But what encouraged us by this is they tended to be shallow and not go deeper. So this gave us good confidence in our capability to locate epicenters with excellent precision inside the network.

Using this model, and the inset map shows the area of this figure, and again the triangle here and here is the NAB station, these events numbered here (Indicating) is what attracted our attention when we used our new velocity model. And again we showed the area ellipsoid axes here, and so it was lineation that attracted our attention.

Now, as good fortune would have it, at about this time Dewey and Gordon with the USGS were on a relocation program for historical shocks in the eastern U.S. It turned out they relocated some six events in the Giles County area. Now, they used joint epicenter location techniques and their analysis was completely separate of ours. And while ours was all micro-earthquakes, their events were all historically felt earthquakes for magnitudes 3 to 4-1/2. Ours were all magnitudes of less than 2, which we classify as micro-earthquakes.

So these are scaled here according to the locational source as well as to magnitude. So four of

Dewey and Gordon's events juxtaposed with ours, and that is what gave us the confidence for the definition of our zone.

Now, since the time this was made we have added several other micro-earthquakes, but they are scattered within here and present nothing new or different.

Now, here is that same suite of data now, again, the same study area at Narrows. And now we have all of the ellipsoidal axes and all of the geography in the area. But the basic reason for showing this is to show the general range of the area ellipsoidal axes. It tends to be on the order of several kilometers.

If we take a vertical section, the triangle again is a vertical section, and take it perpendicular to the zone, we get this kind of distribution here where all of the event numbers and area ellipsoidal axes are shown. And here they are scaled now in terms of source and size.

On two of Dewey and Gordon's events, the stars indicate they had data adequate to locate the epicenter, but not fix the depth. So they arbitrarily set it at one kilometer. But the rest of free depth solutions. So we are looking at these as being essentially from 5 down to about 25 kilometers.

And all of the cross-sections I show, of course, will be one to one, no vertical exaggeration. This is the orthogonal projection now to show the zone on edge. We see this type of distribution in space and here is the distribution with respect to size of event and locational authority.

Now, we have, then two sets of data. There are events located using our velocity model and our network data, and there were events occurring before our network that were located by Dewey and Gordon using their velocity model.

Now, it turned out the Blacksburg station as well as one or two other stations in the region were existing during the entire time frame. So while we used Dewey and Gordon's events falling on top of ours as a supportive argument for our zone, the question could be asked, now suppose you located them all together, at the same time used the joint epicenter determination techniques, if you did them all together at the same time using one velocity model.

That is of course a sound question to ask. But then you run into the question of which event do you want to use for a master event? Do you want to use the biggest event in the zone, off of the zone? Do you want to include off-zone events with on-zone events, and on and on? It turns out there are about five or six reasonable questions like that you can ask.

So we did it, and here are the epicenters for several runs using varying master events and varying subsets of our earthquakes, both on and off of the zone, to produce this type of cluster. So we gridded this and contoured it to get this type of figure.

And our point here now is that no matter what we do with this data, no matter which way we treat it, we end up with the Northeast trending lineation centered in the Perrysburg area. So that is the basis of our contention that even though this is a small number of events, they are high quality and they have been looked at in multiple ways, tested in different ways to establish their validity insofar as is possible.

MR. OKRENT: Two minutes, Mr. Bollinger.

MR. BOLLINGER: Right.

This is the Valiant Ridge province, and the plateau region here and the Blue Ridge here. The black triangle is the Narrows station and the black line is the general extent of the zone we have been talking about. This is to show its disparity with the regional structure and the lineation it tends to exhibit with the central Appalachians mentioned by Bob Hamilton earlier.

With respect to the geology of the region, this is a published geologic section, and this indicates that we feel our events are below this latest detachment fault. And so one particular characteristic of this, in addition to its being below the lowest detachment fault, is that various models give from one-half to one kilometer of a shale unit sitting right on top of a seismic zone.

Now, it would seem to me that modeling could be done, because what you get at the surface motion, I would suspect, would depend strongly on how that thick shale unit would respond to motion on the fault. If it is transparent to it, then of course you would not get the attenuation you would if it tried to participate.

Finally, this slide here, I have also been impacted by the necessity of intersecting structures because, as impressed as I am with my Giles County zone, my geological colleagues think it is a rather stubby little thing. So to attempt to explain how stress might concentrate on it preferentially and also to account for all of these felt events -- these are all felt events -- to attempt to account for those also, there is the possibility of some type of cross-structural feature here that, if this indeed does butt up to it, could explain why you might have preferential concentration here.

Thank you.

(Applause.)

MR. PERKINS: Do you want to reflect on the possibility of there being some kind of connection between this concentrated source and the New River flowing Northwest across the Appalachians? Is there something going on there?

MR. BOLLINGER: Outside of possible fault control for portions of the New River -- that has been suggested several times -- I wouldn't know of any.

MR. HAMILTON: Mark was just asking me if you had any focal mechanisms for this.

(Laughter.)

MR. BOLLINGER: Almost. We have been compositing a focal mechanism and we feel fairly confident of what we have in the sense that we think the Southeast side is down and we think it is probably a reversed fault. But our focal mechanism data are not yet strong enough to define it.

But what we have is in a focal sphere. We have the Southeast quadrant compressional on the lower hemisphere diagram.

VOICE: Excuse me. Do you get nodal plane striping along the same direction as your seismicity?

MR. BOLLINGER: No, no. You see, that is

where we don't have enough data to define the nodal planes. We must use the strike of the epicenters. We do not have enough first motion data to independently constrain the focal mechanisms.

If we assume one of our planes is nearly vertical and striking northeast, then we have a compressional southeast quadrant, which would indicate motion down on the southeast.

MR. OKRENT: Any other questions for Mr. Bollinger?

MR. WENTWORTH: Carl Wentworth, USGS.

Gil, with respect to past discussions and Bob Hamilton's point this morning about pre-existing structures, where else in the East might we expect the kind of circumstances you describe in Giles County?

MR. BOLLINGER: I've been working with a colleague geologist on this. Russ Wheeler is his name. He is with the USGS in the Golden, Colorado, office. We have a professional paper in press now.

But with respect to where else we might find these types of structures, first of all, it seems to us that we need to know more about this particular feature. In other words, does it dip steeper to the northeast or southwest, is it indeed reversed as we expect? And we need that kind of information.

And we also need independent confirmation of the zone. This is all network data. We would like to see reflection seismic work and specifically designed, tailored surfacial geologic work, to get us an independent confirmation of the zone, and to give us some of the details of its geology and its geometry.

Given that kind of information, you are faced with the question of where else. Well, if you say nowhere else, that you think this is a unique kind of feature, then it seems you must appeal to some kind of stress concentrating mechanism, like perhaps the north-northwest linament that I have shown as a combination of intersecting events to get this kind of particular concentration.

So you would have to have that type of situation somewhere else to be able to concentrate the stresses and perhaps get some kind of "uniqueness" that way. If you let it go anywhere and say, well, wherever we have this type of fault striking and it can concentrate stresses under the in situ current stress regime, then it seems to me that you need to delve into the geologic history of this type of fault and try to define geologically regions over which you can expect this class of fault to occur.

In particular, Russ Wheeler's interpretation is that these are reactant normal faults, those are faults associated originally with the opening of the proto-Atlantic and pre-Cambrian or early Cambrian time. So you would need to look at modern analogs to Atlantic seaboard and other types of studies to try to estimate from the American tectonic edge what range or zone you could fine similar features in.

Then that would define the aerial extent wherein you might have a similar reactivation.

MR. OKRENT: We will go on to the next paper, then, which is by Mr. Seeber.

MR. SEEBER: My name is Leonardo Seeber from Lamont Doherty.

Conventional views on tectonics -- and I will elaborate later about what I mean by conventional views -- for interpretive tectonics along the Atlantic seaboard and the Appalachians may be inadequate to explain recent results from new data and from re-examined old data. I will attempt to discuss some of the evidence pertaining to the seismicity studies in the U.S. which are particularly problematic for the conventional model.

To make a case for the new interpretation, I will have to deal with the data in some details, and obviously the data is also in some cases circumstantial because we are dealing with greatinstrumental seismicity in most cases. Perhaps the most critical issue is the size of this earthquake or generally the size of the associated tectonic strength event, which includes the possibility of a seismic component deformation.

Information on the size of an earthquake rupture can be obtained from the amplitude inspection of seismic waves, from the distribution of assigned intensity, and from also the field of permanent information and from the spatial distribution of associated earthquakes. Seismic waves and intensities reflect only on the seismic part of the deformation, whereas strength fields and after-shock distributions will probably reflect the size of the total deformation, including the seismic component if it is present.

I will present data for the research in each of these categories. May I have the first slide, please.

(Slide.)

The first slide deals with the intensity data, and in this slide the data is plotted. The distribution, the area covered by a given intensity, is plotted in this direction and this is the intensity.

Now, you are dealing with a number of different earthquakes. The dark symbols, the black symbols, are for earthquakes in India, and these are all great earthquakes except this, which is an intermediate one.

The open symbols are for earthquakes in North America. Here is the 1906 San Francisco earthquake. All of the others are for the eastern area. Here is the New Madrid earthquake, here is the Charleston earthquake.

If you look at these, this data -- here is the 1886 earthquake. It falls pretty much in the center of the great earthquakes in India. Data on smaller earthquakes for which we know the magnitude and also on the transmission of G and other waves suggests that the attenuation in India is quite similar to the attenuation in the eastern U.S.

From this data we would conclude that the 1886 earthquake would be a great earthquake. However, the 1929 earthquake is plotted also in this figure over here, and it is also essentially indistinguishable from the scatter of the other great earthquakes.

The Grand Banks earthquake was not a great earthquake. It is large, but it is in the intermediate category. This data does not give us a conclusive answer on the problem of how large.

This figure shows the actual distribution of the intensities of the 1886 earthquake, and this is the Grand Banks earthquake. You'll notice the information on the Grand Banks earthquake is not nearly as good as the 1886 earthquake.

This is another way to look at this problem here. We map the data for the 1886 earthquake. This is intensity for essentially the same information you saw on the map in the other direction. These are curves obtained from a model suggested by Everett. This line is from a model of the sub-vertical fault.

These lines are for two models where the fault is much smaller, 20 kilometers sub-vertical fault. You can see that the overall fit is at least equivalent for these two sizes. So you can conclude that you can say very little on the size of the earthquake from just intensity data, and specifically the 1886 earthquake on the basis alone of this data could be a great earthquake.

So to use only intensity data is to throw away a lot of interesting information. It is like having a gourmet meal in front of you and instead of picking up each dish separately you would put everything in a big pile and make a brown soup out of it.

So what I am going to do next is to take the information from each of the kind of effects that were reported and try to deal with that independently. So these slides show what we call the long period effects.

We map here three kinds of effect. S stands for Seish, and it really does not document the Seisch What we are dealing with is records of normal wave activity at the time of the earthquake. I probably can describe it best that way. And one example of that would be up the Hudson River at Kingston waves broke mooring lines for several steamboats and sank a big barge.

The next category to consider is the one indicated by L, which stands for landslides or sink. And the example there that I would quote, in western Maryland an abandoned mine collapsed at the time of the earthquake. There are other similar effects reported.

The third kind is fluid disturbances of the ground, fluid dynamics, and this is mostly from abnormal well activities. An example I put there is a well in Pittsburg that -- several wells in Pittsburg from which gas was extracted. A few hours after the earthquake and it showed an anomalous flow reading, and the day after the earthquake several factories had to shut down because there was not enough gas.

These effects are interesting and the typical effects for great earthquakes. Similar effects have been reported for the '64 Alaska earthquake, for example. So it would point in that direction for the Charleston earthquake.

However, you cannot categorically say that some of these effects would not occur for a medium magnitude earthquake, and we do have to do more homework there. However, a printing in our study of the 1929 Grand Banks earthquake indicates none of these effects were present for that earthquake.

So next I would consider large strain effects, that is the way we have named them, effects in this area. This is South Carolina and the coastal plain, the Piedmont and the Blue Ridge. This is the Cocorp profile. So I would consider reported effects which suggest large deformation.

First of all, the liquefaction. Liquefaction is very strong in the Charleston area. Liquefaction is indicated by L. L with a subscript S indicates a sink, which could be a liquefaction phenomenon or could not be. When you see an L it indicates that actually liquefaction was reported.

At that point there should also be an L. So liquefaction was very strong in the Charleston area, but not exclusively in this area. Liquefaction was reported from this site, this site, from Columbia, South

Carolina, and I guess that's it. So yes, the liquefaction was concentrated there, but it occurred in other places.

This kind of information, liquefaction generates deformation of sediments, but it is not coherent type deformation. So what I consider an axis phenomenon suggests coherent deformation.

I will just run over here and then I will show you slides about the specific area. This was the Charleston area. We were discussing the changes in the grey lines. This is the changes in line of sight from Columbia and Augusta, Georgia. And also there is the failure of a dam in Langley, South Carolina, over in this area, that I will be discussing.

That line is the Augusta fault system. And another important feature I will be talking about a little bit are the extensions reported, extension fissures, dry fissures reported at the failure of the dam here, and also along the fault system.

So this is the Charleston area. Charleston is here. Summarizing this figure is the data that we have on the damage to the railroads, mostly from that. Little patches indicate concentrated liquefaction phenomena.

Most of the damage to the rails consists of both partings and bucklings, that is extension failure and compression failure. However, for a portion of the rails that go from here to about there, about 25 kilometers, the failure was only by compression, by buckling, as you may or may not see on this figure. And this confirms reports found in the newspapers and so on that people extracted from the railroad quite a bit of pieces in order to make them match again.

Also, that draws to scale some of the buckles, and you can undo those buckle and get two to two and a half feet of compression. So we conclude that in this portion of the rail there was a major strain event, compressive strain event in this direction, that is toward the northwest. We give a value of five meters as being a conservative value.

As a comparison, for example, the San Fernando earthquake generated a maximum compression of about two meters. So we think it is not easy, if you want to interpret that as a result of tectonic movement, to fit this kind of displacement to, say, a model of the Charleston earthquake. On the Cook fault that would displace about a meter. You just can't do this kind of surface displacement with that; that is, if you want this to be tectonic.

Next I will say something about the phenomena observed near the fault line, namely the changes in line site at Augusta, Georgia. This is the site. The river comes here, Augusta is here. The section shows where the change of the line of sight was observed.

From the hill, people living up here after the earthquake said the view of the town was greatly enhanced. There are quite a few newspaper accounts of this. We published details on that, and you cannot take it for granted. There are some problems with it, because there are no other indications of deformation in this area. So it's not easy to see how that could happen.

But the accounts are very detailed and seem reliable. So one interpretation of that is that one fault in the Saint Augusta fault system which is drawn here -- this is the location on the slide; it would be over there -- may have had a movement in 1986. There is also another interpretation, which we will be discussing in the next slide, and that is that you have a horizontal movement on the pre-late Cretaceous conformity that has been discussed in other places before.

So at this point I would like to summarize what I said about these large strain effects. This is a section for the Himalayas. I will not say anything about that. But this is a section proposed by Cook and others, the Cocorp data for the Salano River section what I have described as the compression of the rails in this area, the open fissures in this area, and the changes in the line of sight.

Also, we would like to mention relayering, in this area on the Brevard fault has been modeled by Citron and Brown as a normal displacement of about one meter, between 35 and 68. So if you take these for granted -- and I will admit that there is certainly more work to do to be sure of these results -- you would be hard put to explain this kind of effect with the small fault in this region or, I should say, in this region here.

I think that we have proposed that perhaps one possible model is a slip on the detachment or more than one of the detachments that have been by now described, and we will hear more about, I think, by Mr. Beratan in one of the next talks.

So the specifics of the deformation, if you believe the extension in this area and that area, and the compression in that area.

However, we would also like to mention there is another possibility to explain these events, to explain that you have a pressure movement of the sediments, the sedimentary wedge that is over the pre-late Cretaceous conformity. Here you have the feather edge of the coastal plain. This is Langley, where the dam failed. And this is the basement rocks, the Paleozoic rocks in this region.

This unconformity has been described by Pavich and other people to be a buried sacrolite or essentially a very thick clay layer, impermeable. And I think it is conceivable that during the earthquake loadings such a layer with increased core pressure would become less strong, and you could visualize this whole wedge movement with respect to the basement, and creating, of course, pretty dramatic features on the surface like we have described.

So I think that the options are still open as to how to interpret these large strain features. But in any case, whatever model it turns out to be, I think that these effects that we have described, the seismic effect so the '86 earthquake, are very important, first of all because they should be considered in the analysis of the hazard for this area, whatever the model; and also because they suggest that the Charleston type earthquakes will generate structures in these sediments that you should be able to recognize.

In other words, you could go there and do a careful structural analysis of the sediments and perhaps see the signature of previous events. So you can use it as a tool to study the pre-existing seismicity.

So next, the last topic will be the discussion of the seismicity -- I should say, the seismicity subject to the graphs This is the map Mr. Bollinger showed before, and it is based upon essentially felt reports after 1920. It includes also some epicentral locations for the later data.

The next slide is the same region, except this is the instrumental data. I believe Bollinger also showed this slide with essentially the same data. Both sets show broadly the same features, namely you have a seismic zone that runs pretty much along the Appalachians, but the seismicities in southern Appalachia are limited to the northwest of the Brevard Fault, except in essentially two areas: this zone, which is much more evident here than in the previous slide; and then what has been called by Bollinger the South Carolina-Georgia seismic zone.

I will concentrate from now on on this zone (Indicating), which is obviously special, from this data, a very scattered zone of seismicity. You can't define any single fault there for sure. So I think we have a problem right here: What is this zone?

I think some of the problems have been discussed before, just before. People have looked for a geological explanation and geophysical explanation. They have looked for a feature to be assigned to this seismicity. But the consensus so far seems to be there is no such feature.

Therefore, you have to conclude that this seismic zone is essentially a concentration of stress, represents a concentration of stress. So what I will suggest, and I will try to back it up with what I will show next, is that this seismic zone may be a temporary feature, it may not be a stable feature.

From Bollinger's catalogue we are mapping epicenters based upon felt reports for the period 1830 to 1870. That is prior to the Charleston earthquake, and this looks very much like the South Carolina-Georgia seismic zone type of distribution.

In the 15 years prior to the Charleston earthquake, however, the seismicity acquires a peculiar distribution. You have pretty strong activity, but it is surrounding South Carolina. You have very little happening within South Carolina. So this kind of distribution in other areas would lead you to expect a very large earthquake in this area, if you saw something like that happening at a plate boundary in Japan, Alaska, or places like that. So I will refer to that as a *mogadonut, even though I realize that, considering the type, it may be premature to call it that.

This is a summary of the seismicity we mapped here by state and in time from 1830 to essentially the present. So the feature I have just described, the so-called mogadonut, is represented by this burst. What is interesting here is -- it appears in all three states -- the seismicity begins at approximately the same time.

I think we have not done the statistical evaluation of this, but just on an intuitive basis I would say that it looks as if there is a burst of seismicity which is essentially simultaneous in these three states. We have a large burst of seismicity. It makes it a small mogadonut; much more significant, the fact that it appears at the same time over a large area.

Now, continuing along in time, this is the Charleston earthquake. You have a number of foreshocks, then you have the aftershocks. Now, notice, the most prominent feature here is not so much the aftershock but the lack of seismicity, according to this data set in this entire region here, set in the Charleston area.

I should have pointed out, this last line is just the area near Charleston, Somerville, where this is South Carolina, the entire South Carolina except this area.

So it looks like, from this data, that the entire Southeast becomes quiet for quite a long period. So while the activity in Charleston is very high, this seems very strange. No event of this sort has ever been reported in the literature. So considering a number of things, someone said this data referred to the problem of people copying catalogues from other catalogues and so forth.

It turns out that this data is straight from Taylor, and he located all of these earthquakes exclusively on the basis of reports from Charleston or Somerville. And there are a number of other considerations which I cannot get into. But we conclude that in fact this feature is probably artificial, that this seismicity is in fact scattered over a large area, most likely in South Carolina, where you have, according to this data, a complete lack of seismicity for 20 years almost and probably further than that.

So in other words, the seismicity after the Charleston earthquake could very well look in quality, if not quantity, very much like the distribution of the South Carolina-Georgia seismic zone, and is not likely to be concentrated in Charlestown, as shown in this figure.

MR. OKRENT: Mr. Seeber, two minutes.

MR. SEEBER: Yes.

Going along, you have two more bursts of seismicity that appear in this data, and I will show you where they are located, just plotting in the next figure seismicity for each one of these bursts. And you can see that the seismicity in this burst seems to be scattered throughout the South Carolina-Georgia seismic zone in both cases.

So what we conclude is that the seismicity studies in the U.S. can be characterized by large bursts of seismicity that occur over areas of hundreds of kilometers. This kind of time-space solution of earthquakes is not easily reconciled with what I have called before the conventional tectonic models.

This slide should show a little bit of what I mean. On this slide is what I would call the conventional model. You have the seismogenic zone down to a certain depth, whatever it is; a mid-crustal depth; and beyond that depth the fault continues, but you eventually get into a zone where you have continuous creep.

Now, how do you build from this model

seismicity bursts that cover hundreds of kilometers, when this entire depth may be only maximum 40 or 50? Each one of these earthquakes, presumably they are small earthquakes, especially by the data I showed you before, could certainly not create a static field that would trigger an earthquake 100 kilometers away.

So you must invoke, if you believe those bursts, that there is some sort of event that affects this entire area, sort of a ghost behind the scenes that does this whole thing. And I don't see how you can do that in this kind of model, because what you need is something that could act over a very short time. So it is disturbing. I'm not familiar with the kind of processes that could do that.

So I think one way to get around that problem is to suggest the detachment model or consider the detachment model, where it doesn't matter what happens down here. What you have is a seismogenic layer which is essentially riding, decoupled from the subsurface. So if you have maybe a slip on this leg, you can visualize a seismic event passing through rapidly an area and changing the stress field, increasing it and generating seismicity on the layer above.

So this slide summarizes a little bit what I have been talking about. This slide is the kinds of data I have been describing. I should have another data saying intensity data. This is the widespread seismic deformation. This is the seismicity patterns, and this is the long period effects.

This is the kind of model you can adopt for the Charleston earthquake, for example. You can have an intermediate size earthquake with no other effects happening, and if you take these for granted you will not satisfy, you will not be able to generate these effects from that kind of earthquake.

Now, if you have a familiar*earthquake and you trigger say some slip on this layer of the pre-late Cretaceous conformity, you could then generate the widespread large deformation, so you may satisfy that point. But you could not satisfy the widespread burst of seismicity or the long period effects.

The next possibility is you would have an intermediate size earthquake and a large seismic detachment slip. By this detachment I mean the Appalachian detachment. Now, if you have a situation of this sort then you can explain the widespread deformation in this case tectonically if you wish, and you can explain the widespread burst of seismicity as a result of some sort of seismic slip on a detachment.

But you still cannot explain the long period effects if these turn out to be that. You need a great earthquake to produce those effects.

So finally,; you can have a large seismic detaching slip event. In that case, you could then easily explain all of these effects.

So the last slide, I will just briefly say what type of work I think should be done to test these points that we have raised. There is a lot of data in historic or carbon records especially. This data can be used to study the distributions of seismicity, and I would very much like to see that strain effect after the earthquake to be looked into very carefully, what we call the Charleston effect during the aftershock period.

Of course, you could look more into these large strain effect and the long period effects. I would look at the long period effects in a comparative fashion with the '29 earthquake or any other earthquake of intermediate size, because I think the crucial thing to see is whether these effects can be generated by an intermediate size earthquake.

The next type of work I would do to test these ideas is to look at the sediments in the coastal plain. There are a lot of structures there. The type of work that has been going on now for a while in California and the Madrid area, that Dave Russ reported before. By looking at the structure I think you may hope to find something, for example, about the stress in this area, and also perhaps the times involved in these earthquakes.

And finally, the idea that you can have a detached sedimentary wedge above the pre-late Cretaceous nonconformity is an important idea to test, because it makes you understand these effects in terms of the earthquakes and because it has implications for hazard by itself.

I guess I will stop there.

(Applause.)

MR. OKRENT: I wonder if maybe we should take a break now. I am sure there are going to be a lot of discussions about the mechanisms for Charleston and perhaps here at least the various ideas. And if there are specific questions to details of the papers, we can take them up at the time. But try to avoid getting into the broad questions until the various points of view have been given.

In any event, we will take a break for ten minutes, and I will take one or two short questions for Mr. Seeber.

(Recess.)

MR. WENTWORTH: I will describe an argument initially developed several years ago, and it's available in print in a couple of places which I will cite later. The argument is a hypothesis. There is no way at the present time to argue that it is fact, although it is based upon a number of observations.

It proceeds from the geologic perspective as contrasted with the seismologic perspective. This is work I have done together with Marsha Mergner-Kefer, and it represents a compilation and analysis of work by a large number of people. We have examined the origin of a large number of earthquakes along the Eastern seabord using Kenomatic history as our principle guide. We conclude a reasonable and sufficient explanation of these earthquakes is northeast trending reverse fault base.

In a sense, the conventional model that Seeber was contrasting his own arguments with just before the break. To drive these reverse faults, the Atlantic margin seems to have been under northwest-southeast compression for much of the 200 million years since the opening of the Atlantic Ocean.

May I have the first slide? It's there, I see.

By the eastern seaboard we mean the region east of the Appalachian front, that is, the area east of the Appalachian Mountains themselves, extending from Georgia on the southwest to Maine on the northeast. More specifically, in the next several slides we will be looking at this area here (Indicating).

As we are all aware, there are a number of historic earthquakes in the eastern U.S. along the eastern seaboard and in the Appalachians. This is a contour map showing the area of abundance of epicenters from Hadley and Divine.

Our point is simply that earthquakes have occurred along the eastern seaboard and into the Appalachian highlands, although not uniformly so in terms of space. The question, of course, is what causes these earthquakes.

We can seek to explain individual earthquakes or local groups of earthquakes in terms of specific geologic structures. If so, obviously we should do this where possible. But for the most part I think the data are really quite insufficient.

We can also seek to identify whatever broader pattern of deformation may exist, beginning with a premise -- and this is an important point -- that tectonics has regional consistency. If earthquakes along the eastern seaboard have some regional coherence, what is it?

I posed a similar question when the Geological Survey began work on the Charleston earthquake about eight years ago, that is, if we can learn all there is to learn about the 1886 earthquake in South Carolina. Perhaps. But until we can compare the source of that earthquake with similar information about the geology elsewhere, we won't understand the significance of Charleston. Charleston may in fact be tectonically unique or it may only represent one of numerous similar earthquake sources in the region, in the eastern U.S.

Here we have approached the question of earthquake sources along the eastern seaboard from the geologic or Kenomatic viewpoint. Earthquakes represent deformation, and unless each is basically unique through geologic time, geologically recognizable faults should develop as deformation proceeds.

So we begin by asking what the geologic record indicates about deformation in the present tectonic regime. The principal deformation evidence since Atlantic rifting 190 million years ago is subsidance of the continental margin. This has led to accumulation of a wedge of sediment on the continental margin, shown in brown in the cross-section.

The basement surface is bent down two or three kilometers at the edge of the continent and then drops off into the deep marginal basins. To the west, the Appalachians have remained high. Faulting on a major scale has not occurred, except possibly along the front of the Appalachian highlands itself.

More limited reverse faulting has occurred, however, and this attracts us despite its relatively small scale because it is the only widespread faulting that is demonstrable. In summary, the reverse faults trend northeastward, offset the basement surface, the base of the Cretaceous section as much as 100 meters.

They show evidence of progressive movement through time. Most of the documented stratographic evidence for this movement is 50 to 100 million years old, but some more recent offsets are known. Surfacial gravels of offset and earthquake geometry indicate this style of deformation is still under way.

The lower offset rates and limited late Cinazoic offset record make it difficult to establish the movement histories of these faults in the past 40 to 50 million years. This is an important point. Here in this section we are looking southwest near Fredericksburg, Virginia, at the Dumphreys Fault of the Stafford Fault Zone, which has been documented by Nixon and Newell. It sets the basement in pink up against coastal plain sediments in green in reverse fashion, with an aggregate slip of about 35 meters. I have written "kilometers" here.

Let me use that to emphasize a point. These reverse faults have small offsets. We're not looking at West Coast faults. The faults of the Stafford zone show progressive offset through the period 100 to 50 million years ago, but movement was still under way in the past couple million years.

Here we are looking at a drawing of upland gravels offset across the Fall Hills fault, with a displacement of about a third of a meter.

With respect to some of the discussions this morning, it's probably worth looking at this from the point of view of finding such evidence in the geologic record along the eastern coast. There are two colors of lines. One is unfortunately very difficult to see. There is a reddish color and a yellow color.

Reverse faults have been mapped near the fall line in Virginia, there, in Georgia, and have been recognized by seismic profiling in Virginia, South Carolina, and off the coast. And in addition, particularly along the fall line, shown in yellow, there are numerous isolated exposures of reverse faults.

We consider this concentration along the fall line to be fortuitous, rather than to be representative of the real distribution of these faults, and expect more faults to be found elsewhere. And we heard a report this morning of one such discovery.

Farther northeast, the area marked R for Ramapo fault, there is reverse movement along the west out of the Newark Red Bed Basin, as indicated by modern earthquakes. In New England, the Cretaceous and Cinozoicis absent, and this prevents us from recognizing young faults.

The focal mechanisms of new faults indicate that reverse faulting on north-northeast trends is under way here as well. Small movements on faults scattered over a large area raise the possibility of reuse of pre-existing faults, a point Bob Hamilton was emphasizing this morning. Various kinds of faults are possibilities or candidates in the region, but I will focus my attention initially on the early Mesazoic normal faults, and then, if time permits, we should return to this point.

Extension of the crust as Atlantic opening began produced many normal faults with northeast trends, shown here with pink and red, and in New England northerly trends as well. These faults are now recognized principally where they cut early Mesazoic red beds, although they may exist elsewhere as well.

The reverse faults that have been mapped and described in the earthquake focal mechanisms for the suggested reuse of Atlantic normal faults leads us to the following hypothesis, in which we combine information from various parts of the region to produce a coherent behavior for the whole eastern seaboard:

The Atlantic margin is under compression perpendicular to its length and has been for more than 100 million years. This compression has been driving northeast striking reverse faults and still is. Individual faults have been moving at very low rates, less than or equal to about one meter per million years.

The reverse faulting preferentially follows older structures, here represented by early Mesazoic normal faults. Because these normal faults are scattered throughout much of the region, the reverse faults are widely distributed as well, and movement on the reverse faults can generate earthquakes, of which 1886 Charleston is probably an example.

Which of course brings us to the Charleston. We have a lot to learn about Charleston still, but it's interesting to contrast the discussion today for example with the discussion that could have taken place in 1974. We have learned an awful lot.

Work so far indicates that there is in fact northeast-trending structure defined in the basement surface; reverse fault successive movements progressing throughout at least the period 100 to 50 million years ago. These faults probably strike northeastward, and modern earthquakes are occurring in the area of intensity 10 effects to the '86 earthquake.

It's difficult to demonstrate at the present time a specific relation between the modern earthquakes and the source of the 1886 earthquake, in fact maybe an impossibility. If the 1886 earthquake did result from northeast-trending reverse faulting, the available faulting mechanisms suggest the stress field is now at least temporarily different.

Inasmuch as there will be other discussion of Charleston itself, I will pass up any further discussion on this figure or the problem here.

If the hypothesis I have just described is correct and reverse faults scattered throughout the region are responsible for the seismicity in the region, then there should be consistency between the rate of fault movement and the frequency of large earthquakes. We can approach this from the geologic point of view, at least as a consistency test, by using several assumptions.

First, we assume that all offsets on the faults are due to magnitude 7 events. We take the fault rupture length to be 20 kilometers and the average offset per event t be a quarter meter. We model the faults as northeast-trending lines 35 kilometers apart.

This yields a recurrence interval per source area for magnitude 7 events of about 800,000 years, and a frequency of magnitude 7 events for the region of about 10^{-3} per year.

A surprising comparison is that this frequency is identical to a magnitude 7 frequency from magnitude frequency regulations that Dave Perkins has developed for the same area. We conclude, then, that a reverse fault origin for earthquakes along the eastern seaboard is reasonable, that earthquakes like 1886 Charleston should be possible in most parts of the region, that the historic pattern of earthquakes will thus change with time, and that the low rate of movement on individual faults will make identification of specific sources very difficult unless they are currently active.

What I have just done is present to you what I presented at Knoxville last fall.

Now that most of the audience has returned to the room, I would like to point out that the argument I have just described is available to you both in the proceedings volume of the Knoxville conference and in U.S.G.S. Open File Report 81-356.

This argument was developed initially in 1970, late 1978, and a lot of information has been developed since then, some of which reinforces the argument, some of which raises problems for it. The most important problem I am aware of is the work of Nick Radcliff on the Ramapo fault, where he raises a question as to whether the seismicity described by, for example, Aggarwal and Sykes in fact represents movement on the Ramapo fault or the early Mesazoic normal fault, or whether it doesn't necessarily involve some older and perhaps deeper structure.

If Nick is right about this point about the Ramapo fault and if this has general application to Triassic normal faults, then the tie that we use to go from the observed reverse faults to the whole of the region is broken and either that invalidates the argument or we must rebuild it.

One way to rebuild it would be to examine the question of whether in fact the Triassic normal faults aren't following older structures and therefore all of these features have some common pre-Cambrian grandfather or something along that line.

Another idea that's become much more prominent in the last several years is the question of lystric faulting, and if the Triassic normal faults are in fact lystric and if the depth at which they become subhorizontal is not as deep, not well below the depth of the earthquakes, which extend to below 10 kilometers, then again the question is raised as to whether the Triassic faults themselves can be responsible or wholly responsible for the seismicity we are observing, or whether other faults that can extend to greater depths with relatively steep dips aren't involved as well.

A final point, I guess, that I would like to make is to reemphasize that we are looking at a different beast in the eastern U.S. than we are in the western U.S. in terms of rates of deformation. This morning what I consider an important point was made. We don't see extensive fault scarps in the East. The very clear conclusion is that therefore we have low rates of deformation.

If you think about the kind of quaternary record that we would like in order to recognize young faults, you get a good quaternary record where deformation rates are high, where topography is built by tectonics, and since we don't have it in the East, as is well observed, then we don't have a rich quaternary record, so we have difficulty in recognizing young faults in the East.

If we add to that, just by the nature of low deformation rates, that we are looking for very small offsets, it makes it even harder.

Thank you.

(Applause.)

MR. OKRENT: I wonder if you could leave that figure on. I would like to understand something and maybe ask a question. If I understand correctly, you are suggesting that the likelihood of a recurrence of a

Charleston-size earthquake in the entire region under consideration is on the order of 1 in 800 per year; is that right?

MR. WENTWORTH: Yes.

MR. OKRENT: And if I remember correctly, a number like that has been suggested for recurrence of the New Madrid earthquake. Don't you find it a little surprising that in the first 200 years of this nation's life, it has had the earthquakes that should occur on the average of once in 800 years?

MR. WENTWORTH: Yes.

MR. OKRENT: So do I. In fact, it leads me to wonder --

MR. WENTWORTH: What is wrong.

MR. OKRENT: Yes, all right, I will take the word "wrong," even though it is yours. What might be wrong.

MR. WENTWORTH: I don't know. I really don't know. I went through this exercise to examine whether there was any reasonableness in this reverse fault argument at all, and I was startled to find it coming out as close as I did to Dave Perkins' numbers. Perhaps Mark Zoback made a comment about New Madrid this morning which if you follow the implications of it, requires that we are in New Madrid in a period of activity that has recurred in the last hundred million years only three or four or five times, and I find that very difficult to believe.

That is consistent with your question, of course. Beyond that, I don't at the present time see a resolution.

MR. MAXWELL: Isn't the answer on your diagram that says that for the region, one such thing might happen roughly every 1,000 years?

MR. WENTWORTH: That is right, Ben, but we have had one and perhaps two in the last 300, and the question was is the coincidence acceptable, that we happen to have lived in a period where this has happened.

MR. CHINNERY: First of all, even having had two events within 300 years or 1000 years mean return period, the chances of that are quite respectable. I suspect they are about 20 percent on a purely random basis.

MR. OKRENT: It is 200 years, by the way, but go on.

MR. CHINNERY: Even then, the percentage is quite appreciable, the kind of thing one might well bet on in a poker game.

MR. OKRENT: Yes, but we are not playing poker. That is the point.

MR. CHINNERY: But you are assuming we can make these estimates within a factor of 2 or 3, and I don't personally believe any of the numbers you have come up with in terms of recurrence are going to predict with that kind of accuracy, and in that case, really, the problem disappears.

MR. WENTWORTH: Certainly the numbers I have presented are very approximate. There are important assumptions involved that I didn't lay out for you.

MR. OKRENT: But these numbers have a habit of appearing in all kinds of regulatory documents, and it is curious.

VOICE: That is because there is someone on the ACRS who keeps asking for them.

(Laughter.)

MR. OKRENT: But he doesn't necessarily believe them.

(Laughter.)

Questions or comments for Mr. Wentworth?

MR. BAROSH: Pat Barosh, Western Observatory.

One thing, Carl, there are these numbers on reverse faults in the area. There are great numbers of vertical faults elsewhere in the area. We talked about one this morning, the New Shoreham fault. So there are also offshore in the same sequence a number of normal faults. I don't know that there is a preponderance of reverse faults in New England or in the Northeast than vertical or normal, and where we have a Pleistocene movement indicated on faults, that they are normal or vertical.

In the section that you show, in terms of interpreting what kind of movements may have caused those, as a field geologist has pounded on me many times, we say you could not tell from those cross-sections. Pure vertical movements along fronts or ranges will generate a high angle reverse fault. They measure them out as they move upward along any kind of lateral movement. You develop the same sort of feature. There are beautiful flat thrusts that extend out from the San Andreas Fault in a number of places. That you just cannot interpret compression from a very high angle fault, particularly when it is in a whole group, a mixed bag like that. I am just wondering how you had arrived at the conclusion that it was compression.

MR. WENTWORTH: I think if you have a fault that has a geometry that we call reverse, that at the place where that fault is, there has been horizontal shortening across normal to the strike of the fault, and that is compression.

The field observations -- let me say this a different way. There are not a large number of these reverse faults that have been adequately documented from careful field studies. The two that I would site that are the best documented are the Stafford zone in Virginia, documented by Nixon and Newell in Geology several years ago, and the Bell Aire zone in Georgia, documented by Crowell and McConnell, also in Geology several years ago.

For those structures there is no doubt in my mind that we are dealing with reverse faults at the level of exposure at which they are observed. The fault that John Grow described this morning, what did you call it, the New York Bite, New York Bite Fault, and the New Shoreham Fault to the east of that described by McMasters some years ago at GSA are identical in at least these characteristics, with the well-described reverse faults farther south.

They have small aggregate offsets at the base of the cretaceous segment. They have progressive upward movement through time. The problem of distinguishing which direction the fault is from the reflection profile records makes it very difficult to decide whether in those cases they are dealing with reverse or normal faults.

It is clear, I think, from what everyone is saying that an awful lot more work is required before we can be confident -- and certain is probably impossible -- but confident of exactly what is going on. But I am comfortable with the reverse faults as described so far.

MR. BAROSH: I reiterate that the same kind of geometry, not arguing with the observations, but can be formed from pure vertical movement or pure translation, and it commonly is, and can be arrived at from many different field forces. Did you consider the work by Brown, Miller and Swain in their development of the faults during the deposition and their pattern that they felt, mainly lateral fault movement?

MR. WENTWORTH: I tried to attend to the best evidence available, and the interpretations of faults from a small number of drill holes fairly widely spaced in the coastal plains is very difficult. No, I did not try to build a model that attended to those inferred faults.

MR. BAROSH: Or the newer one by Miller across Georgia, which I believe is a graben as he interprets it.

MR. WENTWORTH: Well, it would be consistent to have northwest trending normal faulting.

MR. BAROSH: This is a northeast trending all the way across the coast of Georgia. I believe it is

Miocene.

MR. WENTWORTH: No. I should probably point out the last time I attended to the development of this argument was when I was preparing for Knoxville, which was the better part of a year ago, and that there are a number of things since then that I have not tried to weave into the argument.

MR. BAROSH: And then, not to prolong it, but how long would you concentrate Charleston and the activity where it is where these little faults go all the way along the seaboard? Why would Charleston in that activity be located in that particular position?

MR. WENTWORTH: We just happen to live in a time when that fault went off rather than another one.

MR. BAROSH: And that seismicity tends to go northwestward, looking at the map?

MR. WENTWORTH: Do you mean the so-called South Carolina seismic zone?

MR. BAROSH: Yes, that is at right angles to this.

MR. WENTWORTH: I don't understand the spatial patterns of earthquakes in detail, nor does anyone else. I expect that with time, and that time may be many hundreds of years, but the historic pattern we have got so far will change and change drastically. I really can't comment beyond that because I don't know how it works.

MR. OKRENT: If I could ask a different question, Mr. Seebr suggested that perhaps the Charleston earthquake was a great earthquake. I think that is the word he used. You just put down magnitude 7, thereby, I guess, suggesting you don't think it was a great earthquake.

Do you have any comment one way or the other?

MR. WENTWORTH: I was accepting the views of people who were working on the earthquake at the time I was building this argument. I am really not prepared to comment on that. However, since we brought up the Seeber-Armbruster argument, it is worth noting that the argument I have been describing and the arguments Seeber presented a few minutes ago are not necessarily incompatible, which is not to say I want to adopt a flat thrust, but if there were a flat thrust, the reverse faults are real, they have been observed, they have to be fit into that model, and there could be secondary features which I know Seeber and Armbruster have considered.

MR. SEEBER: I have just a question or two to ask. Perhaps I am asking the question you were asked a few minutes ago, but how do you interpret the South

Carolina - Georgia seismic zone? I don't see a way with your model you could explain this broad seismic zone that is either there now for a while or it is there for a long time or however without any structure associated with it. In your model you really can't produce that.

MR. WENTWORTH: The only way I can see would be some argument that I don't have any foundation for as to the behavior of this place versus that place through time. I would have to call it either fortuitous or some as yet not well-understood behavior of this part of the crust versus that part of the crust through time, which isn't saying much, obviously.

MR. PHILBRICK: Dave, I would like to ask a silly question, if I may. May I?

MR. OKRENT: Go ahead.

MR. PHILBRICK: There was a study made some time ago relating to the length of the fault to the magnitude of the earthquake. All of these little things, how do they fit into that picture that you put on the board showing these short faults in relation to a magnitude 7 or whatever you say Charleston is?

MR. WENTWORTH: For the historic earthquake events with the observed rupture lengths, magnitude 7 can have rupture lengths at least as short as 20 kilometers, and with maximum displacements, at least as small as .8 meter. Those are the figures I used. The reason I used dimensions that are on the small side from the historic data set relate to arguments made by Everington that fault lengths are small in the East, and dimensions suggested by Gill Bollinger from wholly separate kinds of arguments for Charleston.

MR. PHILBRICK: Then there is nothing in the stuff you presented which doesn't fit with that pattern.

MR. WENTWORTH: As long as you are willing to let me take numbers on the low side of the data set, yes.

MR. PHILBRICK: Well, how long were the ruptures in Charleston in '86?

MR. WENTWORTH: There is no observation of fault length in Charleston in 1886.

MR. PHILBRICK: There was a whole lot of ground breakage in Charleston.

MR. WENTWORTH: There were numerous liquefaction features at the surface.

MR. PHILBRICK: Whatever. The surface was broken.

MR. WENTWORTH: But not as a continuous fault rupture which could be considered evidence for the source fault for the earthquake. To my knowledge there has been no such observation East of the Rocky Mountain front.

MR. PHILBRICK: I think you are correct there. I have no problem with that. What you are saying is you can get a magnitude 7 with no ground rupture whatsoever. These new ones that you have are logical.

MR. WENTWORTH: It is important to recognize when you say that for Charleston that there is three-quarters of a kilometer of poorly consolidated sands and shales and that the ground surface in the mithaseismal area is swampy, so that the observations of even a meter of offset at the base of the coastal -- the occurrence of even a meter of offset at the base of the coastal plain section doesn't necessarily mean that at the same time a meter of offset would be, number one, propagated to the surface, and number two, observable there.

MR. PHILBRICK: Because the rock for which the faulting is taking place is subject to compression, and the offset that you had is used up in compression as you go up?

MR. WENTWORTH: I would cast it slightly differently. There is evidence that faulting propagating upward through the coastal plain section degenerates into folding so that at the surface it may not be readily observable.

MR. PHILBRICK: Thank you.

MR. OKRENT: Thank you. I think we had better get on. We will have some general discussion later.

Mr. Gohn.

MR. GOHN: My name is Gregory Gohn, G-o-h-n, and I am a research geologist with the USGS.

I believe I can fairly characterize the other Charleston papers in this section as specific discussions of possible seismicity or seismicity and possible earthquake source structures and mechanisms, and the regional distribution of structures.

I would like to back off a bit in my discussion from talk of the seismicity and source structures per se and rather broaden the topic in a certain sense to a discussion of Charleston seismicity in the context of the tectonic provinces. It occurred to me a minute ago that with the difficulty we have in discussion Charleston seismicity and specific structures, that for the purposes of many of the people in this room, a discussion of Charleston seismicity and tectonic provinces may still be important.

To the extent possible in the time available and to the extent possible from my own personal knowledge, I will attempt to outline the tectonic evolution through time and in a regional context of the Charleston area. I will do that from the point of view of my own point of view, deep drilling and fuel geology.

Although I am not promoting any particular seismotectonic model for Charleston seismicity, I will analyze the major structure associated regionally with each tectonic regime or province, including ancient regimes because of the probable importance of structural heredity and force reactivation to modern seismicity.

I will also try to emphasize the relevance of particular drill hole data to models that have been described by others.

If I could have the first slide.

This is a map showing two circling tectonic provinces with illogic provinces, amorphic provinces but not necessariy seismotectonic provinces. Specifically what you are looking at is a map of the Southeast showing the Atlantic and eastern coastal plains in yellow, and part of the Appalachian origin, probably restricted to the Appalachian Piedmont.

I start with this slide not because I necessarily wanted to. I am going to try to take this geologic history in historic chronologic order. I put it up because at other NRC proceedings I have heard discussions of the coastal plain province and the Piedmont province, or perhaps the Piedmont province as a part of the Appalachian province in a seismotectonic sense.

I think we will see on the following slide that although these are distinct provinces in some senses, they are not distinct provinces in a seismotectonic sense, if one considers that Charleston seismicity occurs below three or four kilometers and the thickness of the coastal plain in South Carolina is probably not much more than one kilometer. The coastal plain is simply a blanket on top of structures at greater depth, and although it may be recording in one fashion or another what is going on, it probably isn't useful to think of it as a seismotectonic province. And in fact we will see that there are some deeper tectonic provinces below the coastal plain which relate in direct or indirect ways to the exposed crystalline rocks at Piedmont.

I might also point out while I have this slide up these are structure contours in meters to the Pre-Cretaceous surface to a very first approximation. It is also an isopach of the coastal plain. I have read discussions of Charleston seismicity in connection with broader features, especially the Cape Fear arch and the Southeast Georgia embayment, also called the Okefenokee embayment, or in some cases the Savannah embayment or East Georgia embayment.

Charleston is here and the 1886 myzoseismal area and the instrumentally recorded datas are here, so the area referred to is just about midway between the Cape Fear arch and the bottom of the southeast Georgia embayment. This is one version. One could probably find several on the geology of the Pre-Cretaceous rocks in the Southeast.

This particular map is an earlier compilation by P. Popano of the USGS as a part of Charleston work. Part of that was included in the Popano-Ziess paper. It is based on drill hole data, aerodynamics and gravity.

To begin our survey of tectonic provinces in the Southeast and also tectonic regimes through time, we can start in the beginning with the Pre-Cambrian. I think I can safely state that no one knows anything about Pre-Cambrian rocks within at least 100 miles of Charleston, and probably one would have to go fairly deep in the Appalachian origin to find Pre-Cambrian rock to look at.

The extent to which any Pre-Cambarian tectonic or lithologic feature may be important is simply beyond our investigation at this point. If we move up in time just a little bit, first the major tectonic occurrence that we can document on a broad regional basis is continental rifting and ocean opening during the late

Protozoic and the early Paleozoic.

Again, when we go to this map we can find virtually nothing to look at and really have no stratographic record, no geologic records. To consider what, if any, meaning this would have to the Charleston area specifically, we would have to again go out into the exposed Appalachians and gather data that might be argued in some regional sense.

Continuing on up into the Paleozoic through the middle and late Paleozoic, we change from extensional and rift to ocean opening tectonics to collisional tectonics in the middle and late Paleozoic. This finally puts some rocks on our map that we can look at.

I am not foolish enough to try to summarize the geologic history of the Appalachian origin in the next five minutes, but I would draw your attention to two areas and sets of features. One, in the exposed Appalachians west of the fault line, which is roughly at the tip of my arrow, this line (indicating), we have the various well-recognized lithotectonic belts of the origin.

A feature I would call your attention to which perhaps has gotten some slight discussion today but maybe not as much as it should is the presence of quite long, probably quite deep and quite significant faults in the Piedmont. These are the major mylonitic, phyllonitic, relatively ductile sheer zones that form the boundaries between major lithotectonic provinces and hence separate rocks of contrasting metamorphic grade, plutonic composition and that sort of thing.

These are very long features. They have been discussed in the context with thrusting, particularly as related to the detachment zone tectonics. They have been discussed as major strike/slip faults, and perhaps in some transitional sense as the major structures of importance in going from Paleozoic depressional tectonics to early Mesozoic extensional tectonics.

The second area I would draw your attention to is the so-called Florida Basement Bloc in south Georgia and northern Florida, the rocks shown in yellows, pinks, blues in this area and this area. Below the Cretaceous coastal plain sediments, these are Ordovician, Silurian and Devonian, well consolidated, generally marine sedimentary rocks. They are virtually unmetamorphosed and virtually undeformed, certainly a contrast to the exposed rocks of the Appalachian and Piedmont.

They have an additional characteristic that when you compare the stratographic to what turn out to the similar kratonic sequences in west central Africa, that is basement cover rocks, there is a striking resemblance. Hence, if one wishes to have a continental collision, large-scale detachment style tectonics, one must integrate this sort of terrain.

Now, what do you have to ask for? I will not attempt to answer this question. Obviously it is out of the zone of intense metamorphism, plutonism and that sort of tectonics in this area, which, incidentally, I should have pointed out. The yellow represents the exposed rocks in the Appalachian origin here, and their likely continuation under the coastal plain some distance south of the fault line, again in yellow, and then a change to a different Pre-Cretaceous province down here.

What do we know about Paleozoic geology in the Charleston area specifically? Again, no drill holes have penetrated Pre-Mesozoic rocks that I know of within 100 kilometers of Charleston, and probably considerably further in certain directions. However, our own USGS clubhouse process No. 3 did go to 1152 meters just west of the 1886 myzoseismal area and bottomed in sedimentary red beds of probable low early Mesozoic age.

The last core taken included a lot of conglomeratic sandstones which contain a large number of detritalithic fragments. This is the only thin thread we have for discussing the Pre-Mesozoic geology directly in the Charleston earthquake area. I have examined these fragments. One finds generally three or four main types of rock.

Far and away the most common is granitic rock, similar to this piece of granitite right here. There are also salt clasts such as this dark one. Much of the light color you see are smaller pieces of quartz or speltphassic, basically plutonic material. One additional type of clast is mylonite, which is not common but which does occur in the deposit, which is almost certainly a representation of considerable stress and response to that stress in a relatively ductile sense by rocks. This would be typical material to make a connection to Paleozoic sheer zones in the exposed Appalachians.

I make the argument in these red beds that because of the poor sorting type of sedimentary structures and general composition and textural immaturity of the product, that the materials you are looking at were very locally derived, and that is the thread that separates these rocks from ones which ones might expect in the basement area of the immediate Charleston region.

As a corollary, one can see these as alluvial fan deposits. Certainly we can think of examples in basin range or in exposed Triassic regions of the East where alluvial fans are related to fault bounded margins indicative of ancient topography produced by the faulting. So again, there is some tenuous suggestion from a sedimentary basis that we are looking at some indication of ancient faulting in the basement.

Moving on with the next major tectonic regime into the early Mesozoic, we changed again into extensional tectonics, continental rifting and ocean opening which led to the modern Atlantic Ocean. Under the Southern Atlantic coastal plain, numerous drill holes have penetrated a sequence of continental red beds which the clubhouse number 3 red beds are an example.

These are typically interpreted as continental deposits within this large green, shown here in green, basis, probably a graben of early Mesozoic age. It appears to connect more successful continental rifting in the Gulf of Mexico with the Atlantic, more successful rifting in the Atlantic Ocean.

Within this same large basin, basaltic rocks are also quite common, such the basalts we encountered in clubhouse crossroads No. 3. This is a piece of core from clubhouse crossroads. We have three drill holes near the 1886 myzoseismic area. The age for these rocks is about 184 million. That is lower Triassic. Beneath it are the red sedimentary rocks of probable lower Mesozoic age, in this case sandstone and basalt stones, and again the same conglomeratic rock.

If you haven't heard already, you will hear quite a bit of discussion about the nature of lower Mesozoic faults in the Charleston area. This is direct stratographic evidence that materials in the case of basalt certainly of that age and in the case of the red bed probably of that age do exist in the area.

The main kind of structures associated with this kind of tectonics are those well known to us all in the exposed Triassic Basin, specifically high angle normal bore faults to basins, similar faults within grabens or basins forming horst and graben sort of structural reliefs within the larger features.

All of these can be seen to a greater or a lesser level of documentation within the large early Mesozoic rifts shown in green on the map. You can see some basement horsts within the larger features that bring Paleozoic crystalline rock directly below the Cretaceous but within the broader belt of early Mesozoic rocks.

Another feature that belongs to this tectonic regime are the widespread sets of sites which are well-known in exposures, but which are widespread under the coastal plane primarily from aeromagnetic data. I mention these because quite obviously this basaltic material is derived from the mantle. It was intruded post collisional tectonics from the Paleozoic. It probably also represents an extensional set of fractures produced during rifting.

The importance here is they could well be the most throughgoing vertical structure in the southeast. They obviously tapped mantle material. They must have had some effect on a horizontal detachment.

Another item which I neglected to point out on the map is the presence of large plutons, those that are -- these are inferred from aeromagnetic and gravity data and coincident highs in both data sets. Those that are most round or most eliptical apparently were used to form, and the largest occurred in the early Mesozoic rift in the northeast.

I mention them again because they have been referred to as possible points of stress concentration within the basin.

MR. OKRENT: Two minutes, Mr. Gohn.

MR. GOHN: One other type of detrital rock found in the Clubhouse Crossroads red beds is another kind of deformed rock, this kind, basalt It is basically the same sort of granitic material we see, granitic classes within the deposits. Some are quite severely deformed in a more brittle manner than were the mylonite class. In this case, you see, it is still essentially a coherent rock, but there is quite a lot of combination of grains in here and a variety of orientations. That is not particularly mineralized. This one is fairly dramatically mineralized by quartz and epidotized, again suggesting, if I can appeal to my arguments for local derivation of this class.

Here is some more direct evidence of rock breakage in the basement very near the Charleston earthquake zone. Neither the mylonitic fabric nor the micropressure fabric extends into the matrix of the closing sedimentary beds, so I am speaking about pre-Mesozoic faulting probably.

Moving then from the rift stage tectonics of the early Mesozoic into the post-rift tectonics or trailing margin or passive margin tectonics of the Crutaceous and Cenozoic, this is the stratographic -- for the coastal plane sediments in Clubhouse Crossroads Number 1. It is here primarily as a reminder to me that one sidelight I will not discuss of the coastal plane investigations we have carried out is the delineation and the importance that has to calibrating geophysical surveys in the area. Obviously, it adds rock strategraphy to seismic strategraphy.

I would like to briefly mention two categories of deformation, Cretaceous and Saleozoic deformation in the coastal plane and Charleston area. Carl Wentworth just discussed most of the features I would mention. Cenozoic faults in South Carolina are of the type elsewhere in the Atlantic coastal plane and fault zone. They are dominantly reverse faults such as just outside South Carolina and Augusta, Georgia, the Bellaire fault. Two other faults in the west central part of the South Carolina coastal plane described by Fay and Prall of the USGS, which apparently were deformed, at least the lower Tertiary sediments, and the faults defined from vibroseis profiles on shore in the Charleston area and by the multichannel reflection lines offshore in the Charleston area which John Berren will discuss in the next paper.

Probably the best known of the faults described from the vibroseis data is the Cook fault. We made an attempt to try to see what the youngest deformation that can be detected is like in the vicinity of this particular structure. What I have done, this is the Cook fault as defined from the vibroseis data. This is the outside of the northwest down to the southeast, northeast striking. What I have shown are points of reflections on reflectors where they steepen down and flatten out again to the southeast.

The data I am going to present, we drilled 450 meter deep core holes, some core, some cuttings, one, two, three, four. There is also a deep water well. Some of the water works here. I will show you the cross-section through SW, Hole Two and Hole One, remembering SW and 2R on the northwest side of the fault as defined by the vibroseis data, and Hole One is on the southeast side.

I apologize for the slide. It is the original compilation made immediately after the holes were drilled. I made a feeble attempt to update the slide by highlighting the entire middle Eocene Tertiary setting, so that the unit marked as L Eocene, Lower Eocene, is also Middle Eocene, now that we have all the fossil data.

To place the Cook fault on the cross-section schematically, the arrow would be the fault that lies between Holes One and Two. It dips to the left, the northwest, and from the vibroseis data the left side is up. Quite obviously you get the exact opposite, since the motion from the stratographic data. The Paleocene section is overly thin on this side. This black clay which I am used to seeing regionally in a lot of wells is not as thick as it normally is. Younger, Upper Paleocene sediments here are not found over here.

My conclusion is that what we are looking at is fundamentally erosional and depositional deposits in which the base of the Middle Eocene section has cut down in this fashion. It does not give you the sense of motion you expected from the deeper structure from the vibroseis data. What can we conclude from this? Either that the Cook fault didn't have a movement history some time after the Paleocene, that rates of movement on that fault were so small that later Tertiary erosional and depositional processes were not controlled by those or possibly that we need to consider something broader as a zone of deformation, and probably a much more complex zone.

There is not a total contradiction in these data in that the highest reflectors in the vibroseis data as the original profiles did not get up this high. Turning it over to drill holes and then going down to the top of the reflectors. In fact, later, high resolution lines run along the same road do show this sort of feature, and one final and quick comment.

As perhaps another avenue approach for getting tectonics in the southeast, it is possible to drill out possible faults much as we have done at the Cook fault.

It is also possible to take a much more regional look at regional marking in the southeast. The quaternary gets most of the play in this regard It is a very, very difficult set of sediments to work with. It certainly is possible to do it with other Sedizoic and Cretaceous elements.

What I am drawing at, for example, this is a nice map of Middle Paleocene marine clay. As you can see the thicknesses in meters, it thins toward the northwest, towards the continent. I infer from this that the depositional trends in the associated shore line was to the northeast, yet if you follow this unit and these isopak lines to the northeast, you get to the upper plenum of the unit, where it is beveled off on a flank of a Cape Fear arch. I would interpret this to mean this unit was once continuous in this direction. Since then uplift in this area has caused erosion of that unit.

If you drop down to the middle Mistrictian, the latest stratographic unit then the Cretaceous in the area, again, as in the previous slide, this is Charleston, this is Summerville, Georgetown, Myrtle Beach, and the coastline. This time, the isopaks tend to trend east-west. You get a thin zone up here where the unit has been beveled by late Cenozoic erosion. You get a thick zone in here which is in the subsurface and hasn't been eroded since some time in the early Paleocene, so it is thicker here and thins in this direction (indicating.)

I would infer from the distribution of this marine unit that depositional strike was this way, more or less subparallel to the axis of the Cape Fear arch. So, we seem to be getting times when this broad positive element was important and deposition at times when it was not. There is not a lot of subsurface data done at this sort of detail, this sort of stratographic detail in the southeast, but it is certainly another avenue of approach.

Thank you.

(Applause.)

MR. OKRENT: We have time for about one question.

MR. POMEROY: Dave, I would like to ask one. I may have lost some of it, because I am not primarily a geologist. But how does this help us in terms of what we are looking for? I mean, I believe in a firm understanding of the tectonics of the southeast, but the question that the subcommittee is hopefully trying to look at, among others, is the question of, is Charleston unique in some way. My suspicion is that if I spent $1 million a year for six, seven, or eight years as we have done in the Charleston area, and another part of the east coast, that we could find some of the possible candidate structures and some of the features that you have described.

Are we getting to where we want to be in terms of describing what it is about Charleston that is unique, if it is, and or what other features in other parts of the eastern seaboard are there that are equivalent? How much more understanding do we need? How much more work do we need in Charleston to solve that problem?

MR. GOHN: It strikes me that you are answering your question as you go along, that you list the amount of money that has been spent, the amount of work that has been done, and are concluding that nothing is unique. I didn't answer your question, but --

MR. POMEROY: Would that be your conclusion, that nothing is unique, that Charleston is no more unique from a geological standpoint than the Norfolk, Virginia, area, say?

MR. GOHN: Again, you pose the other part of the problem. I have nowhere near the knowledge of the Norfolk, Virginia, area that I have of the Charleston area, and I couldn't begin to make a comparison.

MR. OKRENT: Fine. I think we had better go on. Mr. Pomeroy has raised a question that is fairly general in nature, and which I assume will be answered thoroughly this evening, time after time.

(General laughter.)

MR. OKRENT: Mr. Behrendt.

MR. BEHRENDT: I am going to take a crack at the same bit of information we have been talking about all day. You have seen this cartoon this morning in Bob Hamilton's talk, but it summarizes some of the ideas which I think give a hypothesis or several hypotheses for the Charleston earthquake which might also be applied to other areas of the coastal area and continental margin. I will go over this, and then I will present some of the data which led me to this interpretation.

Greg Gohn has just gone over the geology, which saves me a bit of time explaining what we are looking at here. But this cartoon goes from the crust which we see in the Charleston area is banded reflectors up into the area where we are suggesting there is a possible zone of detachment overlaid by high angle faulted terrain listric faults, a high angle at the surface and decreasing down onto the -- perhaps I should have drawn them flattening even more so onto the hypothesized zone of detachment, Triassic basins, also possibly some Durassic rock in this area, the basalt layer, the coastal plane sediments.

Carl Wentworth has referred to the earthquake source associated with the high angle northeast trending reverse angle faults we see very few of in Charleston, and several others up and down the east coast of the United States, and that is one possible sourse of the seismicity. Another would be slipping along this horizontal detachment. To go back in time, I would suggest the zone of detachment originated perhaps at the time of the closing of the Atlantic in the Paleozoic and then at the time of rifting in the Triassic, you had faulting, and the origin of Triassic basins, perhaps localized by earlier zones of weakness. This has been referred to throughout the day, and I think the geophysical data would support this.

Normal faults at that time and now under subsequent to the laying down of the basalt, from shortly thereafter to prior to late Cretaceous time, we had a reversal of stress, and since late Cretaceous time anyway and possibly going back to Triassic time, we have had a compressed horizontal stress in the northwest-southeast direction, causing reverse movement on these faults and perhaps slippage along the zone of detachment.

In case I run out of time, that is my interpretation. This is just a map. We have seen this a number of times today. This is from Barstow and Pomeroy, showing the location of earthquakes in the United States. There is a great clustering around Charleston. The bulk of those are the 1886 earthquake and its aftershocks.

Zobach and Zobach have pointed out in a Science article last year that were not a great earthquake to have occurred at Charleston, there is nothing about the present day seismicity that would lead us to believe it was unusual from other areas of clusters of seismicity in the eastern U. S.

Mark Zobach will discuss this more tomorrow, but I just want to show it as an example of an indication of the compressive stress regime that exists throughout the whole eastern part of the United States and more particularly in the Atlantic coastal area where the northwest-southeast compressive sense, which is both derived from and consistent with the high angle reversed faults that Carl Wentworth has been referring to, but suggesting a regional stress continuous for perhaps the last 100 million years or so.

This is the aeromagnetic map from Zekes and

Gilberts of the Charleston terrain. Here is South Carolina. Charleston is right in this area. Previous interpretations of this have referred to, based on the land magnetics only, the Charleston block. We can see that there is a boundary in this area (indicating) separating low magnetic gradients, terrain, where you find Triassic rifting, from other rocks in the Piedmont, Carolina slate belt and the Charlotte belt to the northwest, and then the Brunswick anomaly here which may contain Triassic rifting sediments, separating this terrain from the south Georgia, north Florida terrain, which probably was a part of Africa at one time. I think Greg referred to some of the totally different geology here from up here.

We see that this low grading terrain continues on beneath the continental margin all the way quite far to the north and is quite common in the areas of rifted Triassic terrain. It may be related to earlier structures as well. In the last three years, partly supported by the Nuclear Regulatory Commission, we have collected about 1,800 kilometers of Bolachow seismic reflection profiling, about 600 kilometers on the land and 1,200 off-shore. It is much greater ease and lower cost of collecting rain data accounts for the wider distribution.

I will be spending the rest of the talk describing some of these results. This is the reflection profile about one second, I believe. I can't read the numbers. But about here. Showing the type of strategraphy that we can see in the acoustics strategraphy correlative with the strategraphy described from the Clubhouse Crossroads well. This reflection comes from what we refer to as J, the pre-Cretaceous unconformity underlying this are coastal plane sediments, late Cretaceous, and Tertiary. The strong reflector has been referred to as K. It is within the Cretaceous under Line J, and this particular record we see evidence of lower Mesozoic, probably Durassic, I would infer, red beds penetrated at the Clubhouse Crossroads well, not very far from this profile. The basalt here was about 250 meters thick, and then there were red beds that were not penetrated completely, and then into another reflector seen weakly here, and more strongly out to the right, referred to as B by Schultz and others, perhaps from crystalline metamorphic terrain, probably of Paleozoic age.

Note that the J reflection is very weak to missing out here. We would infer that the basalt has probably been eroded away here. We are looking at the pre-Cretaceous unconformity in this area represented probably by the Triassic sediments.

This is immediately off-shore to the Charleston area. Here we see the J reflector continuing across, but in this case, well, we also see a Triassic basin here, no evidence of any offset in the beds above the boundaries of these boundary faults. This is a couple of kilometers deep. Considering the more than twice velocity of the sedimentary rock beneath this reflection compared to that above the coastal plane sediments.

Note that the J reflection probably directly overlies crystalline terrain here. This is consistent with the refraction velocities of pr six plus kilometers per second off-shore compared with a 5.5, 5.8 on land and probably 4.5 refraction velocities over the Triassic red beds.

So we have made a contour map of the basalt surface. It is very smooth. A dome is found extending at least -- making a smaller -- of the basalt fields in the well, but in the area of the Charleston earthquake, the upper central Meizoseismal area is here, and the epicenters of recent seismicity are shown in black dots, and in the area of faults that we have been discussing, it is very smooth and very flat, and as Bob Hamilton emphasized this morning, there -- features of the surface. The reflection records I am showing you are the anomalous ones. In fact, I could bore you with slide after slide of very smooth, flat reflectors.

A little more detailed contour map also based on some more recent data in the epicentral area and Mesoseismal area, the inferred Mesoseismal area of the 1886 earthquake, and we see the details. These contours are in tenths of a second, since the velocity is close to 2. kilometers per second. We see this gradual drop, about three-tenths of a degree, but steepening in the area of the Mesoseismal area of the 1886 earthquake, and the areas where the small faults which have been referred to all day today with these high angle reverse faults have been detected and where modern epicenters are. This is a zone flecture of about 200 meters relief, which we refer to as the Summerville flecture.

The town of Summerville is right in the middle of the seismic lines, which accounts for the sparsity of seismic data in perhaps the most interesting area. This has a northeast trend and probably represents draping over the deeper structures that may have been associated with it within the basement.

Hans Ackerman has done seismic refraction work in the area, and also prior to the reflection work, and has defined this same flecture in the pre-Cretaceous unconformity determined from the refraction results, and he has also defined a Horst structure, a Horst and Graben structure interpreted as a boundary of a Triassic basin here, based on seismic velocities and the depths.

You can see the faults I have been referring to superimposed. We have two reflection profiles across the edge of this Graben. I will show one. This is called the Gants fault. It has a northeast trend. This is the profile across it. This should really, if I back up, you can see the trend of the seismic profile almost north-south. All right. I would interpret this as indicating these reflections come from sedimentary rocks, probably Durassic or Triassic sedimentary rocks. I would infer the existence of a fault in here. Probably, if we correlate this reflection with this down here, it would indicate about a kilometer of throw which is consistent with the refraction data.

If one wanted to be more adventurous, you might try to correlate some of these with something like this in any case, having a normal fault along this trend, indicating a tensional feature downdropped to the right, subsequent to the laying down of the basalt. It was exposed for perhaps close to 100 million years, and presumably eroded smooth, then covered with late Cretaceous rocks, and the coastal planes sediments.

Then we have compressional tectonics, reversing the movement on the normal fault, now giving us the flectures you see above, not actually breaking the basalt, probably, and certainly not the coastal plane sediments, but indicating reverse movement on the bounding fault now becoming a high angle reverse fault in the upper part of the section, and of course there is only about 50 meters displacement on the basalt reflector here, and subsequent less movement progressively up the section, indicating the movement through time.

MR. OKRENT: Two minutes, Mr. Behrendt.

MR. BEHRENDT: Also, we have another fall to the Helena Banks fault off-shore, and we can see evidence of this. This is at least 50 kilometers long. It is possible it could be a source of the Charleston earthquake. Looking at the upper tens of meters, we ca follow the Helena Banks fault up within ten meters of the sea bottom. We also have deep reflection data recorded off-shore and we could see this extending to 12 seconds on this record, reflections from the mantle. We have about 450 kilometers of this data off-shore with basalt reflector up here and the coastal plane sediments, but also a series of defractions which you probably cannot see very well on this slide, but I will show a magnification of this same data at three times the vertical scale.

You see these, we infer these come from some type of rough spot. That is something that can produce a defraction. Not one necessarily above the other, but both perhaps out of the plane of the section with this being closest to vertically beneath the surface.

Showing a line drawing of two of the profiles, we see this surface at about three seconds continuing. These two lines cross, but we have a whole grid of lines that shows these, and I attempted to make a contour map to the surface of the tops of these, but as you can see, if you would have time to study it, basically they are all at about the same surface, 3.7 plus or minus a half a second, which would correspond based on refraction velocities to 11.4 plus or minus a kilometer and a half.

That would correspond, if we assumed, to be conservative, we would take -- assume some of these are out of the plane of this section, and might come to the surface at about ten kilometers' depth. This is the hypocenter determined from the seismic net by Tyre and Rank, and you can see that if we projected the ten kilometer deep surface into the area of epicenters, we would see that it extended within the hypocenter depth, so this would not be inconsistent with the movement of a long horizontal plane.

To check this interpretation on land we ran a profile across the state of South Carolina -- it will be completed across the entire state -- recorded to eight seconds' depth, in the area of the Mesoseismal area close to it. These are the coastal plane sediments. This is a line drawn from this (indicating). We didn't see the quality of the data we saw off-shore, probably because we were using 2,000 cubic inch airguns off-shore. Here we are using four vibrators, and we don't seem to get, because of the coastal plane sediments, the penetration, but we do see some suggestion of the defractions at about the three-second depth, and the short segments of reflection at about the same depth. These have been duplicated on a COCORP line along the same track. So we know this horizon is quite real even though we cannot follow it.

Farther landward, farther into the state of South Carolina, we see evidence of another Triassic basin, and incidentally, this is near the Bowen area of seismicity. There is also small earthquakes recorded not far from this Triassic basin. We also see deeper features in this one. If one wanted to believe this feature here (indicating), it would project to the surface near the trend of the Augusta fault in Georgia.

In fact, of course, if this were properly migrated, the feature here for instance would move way up, so we would not suggest there is anything here on the sloping feature of seven seconds. However, these other features are at this depth. Here is an example of one of those (indicating). This would correspond with about 20 kilometers' depth, so we do see segments of features in the crust even below the coastal plane sediments. Here is one of the curved defractions (indicating).

Then, crossing the coastal plane sediments and getting into the slate belt, we begin to see much more in the upper part of this section, perhaps geologically different, but also perhaps because of better energy coupling. Note the absence of any J reflection or any coastal plane sediments at the top. We see features here and in the upper one or two seconds and also some deeper features that are four and down to six seconds, and just an example of the much greater information in this part of the seismic record section, note the change in the magnetic pattern. We are now across that boundary, out of the Charleston terrain, and into the Piedmont and magnetic characteristics of that.

If one wanted to suggest that this detachment surface or decollement continued based upon these data, first of all, we can see a continuous reflection all across South Carolina, but we do see these features occurring here, occuring at about one and a half seconds, maybe a three to four kilometer range, compared to about ten kilometers in the Charleston area.

I would hesitate on the basis of these data not to connect them, but that could be possible, because we have a poorer quality data beneath the coast. Note the change in quality when you go from the coastal plane sediments into the piedmont.

Thank you.

(Applause.)

NUCLEAR REGULATORY COMMISSION

This is to certify that the attached proceedings before the

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in the matter of: ACRS/Subcommittee on Extreme External Phenomena

Date of Proceeding: January 28, 1982

Docket Number: ______________________

Place of Proceeding: Washington, D. C.

were held as herein appears, and that this is the original transcript thereof for the file of the Commission.

Sharon Filipour

Official Reporter (Typed)

Sharon Filipour

Official Reporter (Signature)